

# QUARTERLY SUMMARY

## OF THE

# IMPROVEMENTS AND DISCOVERIES

## IN THE

# MEDICAL SCIENCES.

### ANATOMY AND PHYSIOLOGY.

1. *On the separate course of Sensation, Volition, and the Involuntary Power from and to the Brain, the Oblong Medulla, and the Spinal Cord.* By JOSEPH SWAN.—The oblong medulla may be divided into four principal parts. The anterior central part forms the pyramidal bodies; these, with the crura of the brain, give origin to several motive nerves, and may be called the voluntary tract. The posterior surface forms a layer which contains the floor of the fourth ventricle, and forms a bed for the origin of the auditory nerve, and a great portion of the fifth; this may be called the sensitive tract. Between the two preceding tracts there is a large ganglion or case, filled with gray and white matter; and this may be called the involuntary tract. Besides these, there are at the sides the restiform bodies, principally derived from the cerebellum; but it is not intended further to notice them.

The sensitive tract in the oblong medulla is a layer of moderate thickness. It passes upwards as far as the nates, and then obliquely across and underneath these to the inner and posterior margin of the thalamus; it here forms a cord as thick as a swan's quill. It passes outwardly across the thalamus, underneath the optic tract; it sends off a layer of wavy tracts or lines underneath the epithelium of the posterior and descending horns of the lateral ventricle, and is then intermixed with the white matter of the posterior lobe of the brain.

The voluntary tract, after forming layers in the annular tubercle, and combining with the inferior pedicle of the cerebellum, forms the external portion of the crus of the brain, especially that bounded by the involuntary tract and the black matter. The larger part of it passes upwards, communicating freely with the middle and broad portions of the striated body, in its course to the white matter leading to the great commissure and the convolutions of the brain.

The involuntary tract forms, in the oblong medulla, between the sensitive and voluntary tracts, a broad and thick triangular and somewhat heart-shaped ganglion or case, having partitions filled with gray and white matter; below, it sends processes downwards, to pass at the bottom of the longitudinal fissure between the two anterior quarters of the spinal cord. Opposite the crura of the brain it divides into two portions, each of which is a cylinder, containing pouches filled with gray and white matter. On arriving at the thalamus, each passes outwardly and anteriorly to the sensitive tract; at this part the cylinder forms a distinct ring, from which the involuntary tract is continued across the thalamus, between the sensitive and voluntary tracts; and, after communicating with the narrow end of the striated body, becomes combined with the white matter leading to the great commissure and the convolutions of the brain.

In the horse, ox, pig, and sheep, the sensitive tract passes from the posterior surface of the oblong medulla nearly as in man; it forms an expansion underneath the epithelium lining the posterior horn of the lateral ventricle, and the

rest is combined with the white matter corresponding with that in the posterior lobe of the brain in man.

The voluntary tract in animals is very similar to the same in man, except that it undergoes a more simple change in the smaller annular tubercle on becoming combined with the inferior pedicles of the cerebellum.

The involuntary tract in animals is a most curious and interesting structure. It is firmer than any other part of the brain, and, in external appearance, very much resembles a ganglion of the sympathetic nerve. It begins in the thalamus, at the ring by which the soft commissures communicate. It is surrounded by the third ventricle, and its continuation downwards lies beneath the passage from the third to the fourth ventricle. In the fourth ventricle it lies immediately underneath the central part of the sensitive tract. At the ring it forms a large, round, or oval case of white matter; this is composed in the centre of oblong cells, plainly distinguished by the naked eye in the horse; and into these the gray matter of the soft commissure enters for connecting their two sides. The ring is very large in the horse; it is connected to the nucleus of the white matter of the brain anteriorly, and with the tænia, and through this with the sensitive tract. From the posterior and outer side of this case white fibres pass off, which go through the narrow end of the striated body to the white matter leading to the convolutions; these fibres correspond with the same termination of the involuntary tract in man. Both sides of the lower end of the case become concentrated in a large ganglion of the size of half a swan's quill; this is soon joined by a similar ganglion from the opposite case. The ganglion thus formed contracts to the size of a goose's quill; it then enlarges again in the oblong medulla, and assumes an oblong and pyriform shape, the narrow end being continued into the spinal cord, where it lies in the bottom of the longitudinal fissure formed by the two anterior quarters. The case and ganglions communicate by numerous fibres, especially laterally, with the contiguous parts of the brain, and with the oblong medulla and spinal cord. In the ox the case and ganglion are similar, but less. In the sheep they are also similar, but less than in the ox. In the pig the same parts are also similar; the ganglia are not quite so large as in the ox, and are less oval in shape. This large ganglionic mass appears as if it were the source of vital power—for giving energy to all the other parts—for exciting involuntary action constantly in some parts through the fibres it sends through the narrow end of the striated body—and for calling into action all the other parts, when any necessity arises for their assistance.

This brief paper is the result of very numerous dissections, and is an outline of my very constant occupation for many months.—*London Medical Gazette*, Feb. 1851.

2. *New Muscular System discovered in the Mucous Membrane of the Stomach and Intestines.* By ERNST BRUECKE, Prof. Phys. Univ. Vienna. (Communicated by Prof. Van der Byl).—At a meeting of the Imperial Academy of Sciences of Vienna, held on the 13th February, 1851, Professor Bruecke communicated his discovery of a muscular system in the mucous membrane of the stomach and intestines. In examining the intestinal villi of fowls, geese, dogs, and human subjects, with reference to the origin of the chyle vessels, he met with a system of longitudinal fibres, which in their morphological characters were found by him to correspond exactly with organic muscular fibres. He now remembered that, already in the years 1842 and 1843, Lacaze, Gruby, and Delafond (from observations made by Lacaze on recently killed animals, and by Gruby and Delafond on living ones), attributed a power of motion to the intestinal villi—and therefore sought to convince himself of the correctness of this statement.

After narcotizing a dog, according to the method proposed by Professor Ludwig of Zurich—by injecting a strong solution of the diluted alcoholic extract of opium into the jugular vein until the animal was entirely insensible—Professor Bruecke laid open the abdominal cavity by cutting through the linea alba, and then opened the small intestine in several places. On now drawing a thin blunt probe very lightly, but somewhat rapidly, across the mucous membrane, he dis-

tinently saw the villi which had been touched shortening and thickening themselves. Professor Bruecke did not see them become long and slender on irritation, as Gruby and Delafond pretend also to have observed; and he doubts this change of shape as an active condition, since he was unable to find any circular fibres in the villi. Nor did the lateral motions take place, as described by Gruby and Delafond; but he does not deny their possibility, since they might be produced by the fibres on one side of a villus contracting, while those on the other side remain relaxed.

Professor Bruecke, now no longer doubting the nature of the fibres observed by him, sought to investigate their extent and attachment in the intestinal canal of human subjects and dogs, and found that in the villi he had observed only the most remote tendrils (*Ausläufer*) of an expansive muscular system which extended through the mucous membrane of the stomach and intestines.

The principal layer of this muscular system is situated in the stomach below the pepsin glands—in the small intestine, below the Lieberkühnian follicles—in the great intestine, below the *glandulae tubulatae* (*simplices minores* of Böhm), and consists of an external layer of longitudinal and an internal layer of circular fibres. Situated on the inner surface of the latter (or internal) layer, we have an irregular meshwork of these fibres, which surround the base of the above-named glands, and pass between them to reach the very surface of the mucous membrane, where they are only covered by the epithelium, basement membrane, and capillary vessels. It is to this meshwork and its tendrils that the villi owe their contractile powers.

*Note by Dr. Van der Byl.*—In sending this communication, I may add that, through the kindness of Professor Bruecke, I have had an opportunity of examining this new muscular system in the stomach and intestines, and convincing myself of the contractile power of the villi. The following remarks may perhaps aid those who are willing to repeat the observations described in this paper:—

1. *To observe the Contractions of the Villi.*—The intestine being slit open, we must not rest disappointed if the contractions do not take place immediately. In the last dog experimented on by Professor Bruecke, the contractions of the villi became strongest about a quarter of an hour after the internal surface of the intestine had been exposed to the air; and, indeed, they may not set in until twenty minutes or half an hour after they are laid open. The contractions are not to be expected where the surface is granular (for here the villi are already contracted, as we see from their thick round apices); on the contrary, the florid velvet-like surface, covered by a thin layer of transparent mucus, will exhibit the contractile power of the villi most satisfactorily.

2. *To demonstrate the Muscular Fibres in the Fresh Villi.*—Remove a few villi from the intestinal surface, and spread them out on a glass (dissecting them a little, if necessary). On examination with the microscope, we usually find some broken through, and see the fibres hanging from the broken extremity. With a high power, we can then examine the fibres individually.

3. *To demonstrate the Layers of this Muscular System.*—For this purpose it is necessary to boil the stomach and intestines in dilute acetic acid, or common vinegar, after the manner proposed by Purkinje, many years ago, for other purposes. When the organs are very thin, we wait until the acid boils, and then dip them in for a few minutes;—thick organs, however, are put in when the acid is placed on the fire, and removed as soon as it boils. After such boiling, the stomach and intestines are spread out, and allowed to dry moderately. When too dry, they become very brittle, and it is then extremely difficult to get good sections. Since the layers are comparatively very thin, for their better observation the sections should be made obliquely, not perpendicularly, to obtain a greater surface.—*Monthly Journ. Med. Sci.*, April, 1851.

3. *Experiments on the Pancreatic Fluid of the Calf.* By M. J. L. LASSAIGNE.—Since it has been shown that the pancreatic fluid extracted from the dog enjoys the remarkable property of making an emulsion with oil, and of causing its transformation into fatty acids, either at ordinary temperatures or at the temperatures of the bodies of mammalia, it has become matter of interest to submit

to similar experiments the pancreatic fluid obtained from a large herbivorous animal, with the view of ascertaining if it acts in the same manner.

M. Colin, director of the anatomical department of the school of Alfort, having procured some of it, by the method shown to him by M. Bernard, and which he had successfully put in practice with the dog, a small quantity was sent to M. Lassaigne, which he immediately examined.

The pancreatic fluid of the calf, which has an amber yellow colour, is inodorous, is transparent and limpid, and possesses a tolerably marked saline taste; when tested with reddened litmus paper, it restores the blue tint. Heated, it becomes opalescent and turbid, and deposits some coagulated white flakes, which attach themselves to the sides of the vessel in which the experiment is made.

The small quantity at the author's disposal not permitting of a direct analysis, he thought it right to examine, in concert with M. Colin, the action which it exercises on oils.

For this purpose he divided it into several portions in glass tubes, and added to each portion a drop of pure olive oil. The agitation to which he subjected the tubes formed no emulsion—the oil separating speedily, and floating on the top.

The prolongation of the experiment did not produce any effect, even after eight hours of contact at a temperature of 60° Fahr., the oil having remained neutral, and the liquid preserving its primitive alkalinity. Subsequently exposing these same tubes in a salt-water bath to 100° Fahr., and keeping up this temperature for fourteen hours, produced no change in the properties of the oil, or in those of the pancreatic juice subjected to the experiment.

This result is opposed to that obtained from the pancreatic juice of the dog,—is it constant in the herbivorous animal, or is it an exceptional occurrence due to an alteration in the pancreatic juice of the animal, caused by the painful operation to which it had been subjected? The author does not admit the latter hypothesis, seeing that the animal survived, and did not appear to suffer in health.—*Monthly Journ.*, May 1851, from *Journ. de Pharmacie*, March, 1851.

**4. Further Experiments on the Pancreatic Juice of the Dog.** By M. J. L. LASSAIGNE.—We know, from the interesting experiments of M. Bernard and Barreswil, that the pancreatic juice possesses the remarkable property of forming an emulsion with fatty bodies, either vegetable or animal, and of transforming them into fatty acids and glycerine, at the temperature of from 95° to 100° Fahr.

M. Bernard, in consequence repeating, at the school of Alfort, along with M. Colin, the process by the aid of which he obtained the pancreatic fluid from animals, remitted to M. Lassaigne a portion of this juice, which he had extracted himself from a dog of middle size.

The experiments to which he submitted it allow him to add some facts to those already known, and of which the principal may be stated in the following propositions:—

1. The action of the pancreatic juice upon oils is produced even at the temperature of 53° to 60° Fahr. in some hours. In fact, on moistening several points of blue litmus paper with the emulsion produced by the olive oil and pancreatic juice, the moistened portions of the blue paper reddened shortly, from the circumference to the centre, on exposing to the air; and to produce this effect, it was not found necessary to leave the mixture during twelve or fourteen hours at the temperature of the bodies of mammalia, as has been advanced by the authors quoted above.

2. The mixture of the pancreatic juice and oil becomes acid at ordinary temperature, as the author has ascertained after an experiment of similar duration.

3. This acidification is produced in oxygen, air, hydrogen, nitrogen, and carbonic oxide gases; the air does not seem to participate in this singular change, which is perhaps due to a force of the same nature as that designated, by Berzelius, "catalytic force," to explain certain reactions in organic and inorganic chemistry.

4. The pancreatic juice can preserve its alkalinity and property of acting on oil for several days.

5. Under the same circumstances in which the oil is modified by this fluid, sugar and gum undergo no alteration, a circumstance which points out the speciality of the action which it exercises on fatty bodies.—*Ibid.*

5. *Absence of the Sternum in a Child aged Eleven Years.*—An instance of this raro deformity is related in the *Zeitschrift für die Gesamte Medicin.*, by Dr. LOUIS BENJAMIN. The sternum was replaced by two long narrow cartilaginous bands, commencing at the clavicle, and extending to the level of the seventh pair of ribs. Between the two bands was a space occupied solely by the soft parts, and which was perceptibly elevated at each systole of the heart. The child's health was remarkably good, and the respiratory functions were performed as in health.—*Prov. Med. and Surg. Journ.*, May 28th, 1851.

6. *Total Absence of Uterus.*—Dr. ZIENL records (*Medicinisches Correspondenz Blatt*) an example of this in a woman fifty-seven years of age, who had been married at thirty two, and until the latter years of her life had enjoyed excellent health. She had never menstruated, but had at times a mucous discharge from her vagina. Her body was well formed, breasts well developed, her voice and manners those of her sex. Coitus had never been completely accomplished, and she was indifferent to it. She died of phthisis.

On examination, not the slightest trace of a uterus could be found. The external organs were well developed. The vagina was so contracted as scarcely to admit the index finger, and terminated at the extent of an inch in a cul-de-sac. The Fallopian tubes were in the broad ligaments; the sibilated extremities were normal; the abdominal opening of the tubes was open; there was no uterine opening. The ovaries were firm and dry, puckered on their surface, and containing in their interior small compact protuberances.—*Gaz. Méd. de Paris*, Jan. 4, 1851.

7. *On the Parovarium of the Female, the Analogue of the Epididymis in the Male.* By Professor KÖNIG.—The Wolffian bodies are generally believed to be peculiar to intra-uterine life, and to disappear in both sexes without leaving any trace of their existence. The author, however, affirms that it is possible to demonstrate anatomically, that these organs in both sexes, generally regarded as temporary, continue throughout life, and that they do not begin to waste until after the cessation of the generative functions. The following paragraphs contain the pith of this interesting essay:—

1. In the earliest periods of intra-uterine life there is no distinction of sex, the elements of both sexes in fact existing in every individual.

2. The "generative gland" may become either a testicle or an ovary.

3. By means of the excretory ducts of this gland, that is to say, by the canal of Müller in the female, and the excretory duct of the Wolffian body in the male, each embryo assumes the attributes of one or the other sex.

4. The sexual difference consists in the development of one of these ducts, and the arrest of development in the other.

5. In the male, the Wolffian body does not disappear entirely, but it constitutes in chief part the epididymis; its cul-de-sac forming the *coni vasculosi* of the epididymis.

6. The superior cul-de-sac of the excretory duct of the Wolffian body become effaced, or converted into hydatid vesicles. The inferior cul-de-sac also disappear, or are converted into *caso aberrantia Halleri*.

7. The excretory duct of the Wolffian body forms the canal of the epididymis and vns deferens.

8. In the female the Wolffian body never entirely disappears, but it contributes to the formation of a new body, discovered by Kobelt, and called by him the *parovarium*. This body is placed between the ovary and the Fallopian tube.

9. The cul-de-sac disappear or become *coni vasculosi* as in the male, others become the vesicles so often observed in the fringes of the Fallopian tube; others, again, the inferior ones, disappear altogether.

10. The excretory duct of the Wolffian body is arrested in its development. The duct of Müller becomes the Fallopian tube.

11. Hermaphroditism is caused by the persistence of all the elements of the generative tissues.

[In connection with the above we may append the conclusion respecting the Wolfian body, extracted from an inaugural thesis, by Dr. Follier:—]

The Wolfian bodies are developed in two distinct portions on each side of the spine, originating in special organic cellules, and not as an expansion of either of the allantois or of the vesicula umbilicalis. The generative gland (either ovary or testicle, as the case may be) is formed quite independently of the Wolfian bodies. These bodies are formed of tubes, at first straight, but afterwards becoming tortuous, and ending in an excretory duct. The excretory duct of the generative gland is developed independently of that of the Wolfian bodies, which bodies are not completely effaced at birth. In the female they persist partly in the form of small canals situated in the broad ligament, near the ovary, and in some instances appear in small cysts. In the male the remains of the body are found near the head of the epididymis, and consist of small diverticular canals, the *vasa aberrantia Halleri*, and the hydatid of Morgagni. The author denies that, as Kobelt asserts, the canalicules of the Wolfian body form the vasa efferentia of the testicle.—*Provincial Med. and Surg. Journ.*, May 14th, from *Recherches sur les Corps de Wolff*. 1850.

#### ORGANIC CHEMISTRY.

8. *On so-called Chylous Urine.* By H. BENGE JONES, M.D., &c.—The definition given of chylous urina is, that it is urine which is white from the suspension of fatty matter in it. An opportunity of observing a case of this disease having occurred to the author, he was led to make the experiments described in this paper. A baracca-maker, æt. 32, half-caste, who had lived in London for twelve years, had been passing such water for nine months. On examination of the water made at 2 P.M. it solidified, looking, in ten minutes, like blanc-mange. It was very feebly acid, contained fibrin, albumen, blood-globules, and fat; specific gravity = 1015. 1000 grs. of this urina gave—

44.42	grs. total solid residue.
8.01	grs. total ash.
14.03	grs. albumen.
8.37	grs. fat.
13.26	grs. urea and extractive matter.
.75	grs. loes.
355.58	grs. water.

In order to watch the variations produced by food and exercise in the appearance of the urine, every time the urine was made, for five days and nights, it was passed into bottles marked with the hour. From these observations, and more particularly from the third, fourth, and sixth days, it was evident that the fibrin and albumen appear in the urine when no fat is there, and that the albuminous urine occurs before food has been taken, and disappears during the night with perfect rest. Thus, the fourth day, at 7h. 15m. A.M., on first getting up, the urine contained the slightest trace of albumen. The specific gravity = 1027; the precipitate by alcohol = 0.8 gr. per 1000 grs. urine.

At 9h. 50m. A.M., just before breakfast, the urine formed a solid conglom., free from fatty matter, but contained a visible deposit of blood. Specific gravity = 1015.6; the precipitate by alcohol = 14.1 grs. per 1000 grs. of urine.

At 11 A.M., the urine was chylous or white from fatty matter.

Further experiments on the influence of rest and motion in lessening or increasing the albumen in the urine previous to food were then given.

On five different mornings, by rising early or late, and by collecting the precipitate from the urine by alcohol, the influence of rest and motion was determined. The author states that he could fix beforehand whether the urine should be albuminous or not, by directing the patient to get up, or to lie still.

The patient was bled, and the serum was opalescent, but did not clear with ether; the blood contained no excess of fat. 1000 parts of blood gave—

2.63	grs. fibrin.
159.3	grs. blood-globules.
78.1	grs. solids of serum.
240.03	grs. total residue.
759.97	grs. water.

The urine made the same day was examined at different hours; that made immediately before the bleeding was quite white, and that made an hour and a half afterwards was very milky also. Specific gravity = 1018. 1000 grs. of urine gave—

56.87	grs. total residue.
10.80	grs. total ash.
13.95	grs. albumen.
7.46	grs. fat.
24.06	grs. urea, &c.
.60	gr. loss.
943.13	grs. water.

The conclusions from these experiments are—

1. That so-called chylous urine, besides fat, may contain albumen, fibrin, and healthy blood-globules.

2. That, although the fat passes off in the urine after food is taken, yet the albumen, fibrin, and blood-globules are thrown out before any food has been taken. During perfect rest, the albumen ceases to be excreted; and it does not appear in quantity in the urine even after food is taken, provided there is perfect rest. A short time after rising early, the urine may coagulate spontaneously, although no fat is present: and this may happen previous to food, when the urine is free from fat.

3. Though the urine made just before and a short time after bleeding was as milky as it usually was at that hour of the day, yet the serum of the blood was not milky: it did not contain a larger quantity of fat than healthy blood does.

The general results are—

1. That the most important changes in the urine in this disease take place independently of the influence of digestion.

2. That the urine in one respect only resembles chyle, and that is in containing, after digestion, a large quantity of fat in a very fine state of division. The supposition that the disease consists in an accumulation of fat in the blood, which is thrown out by the kidneys, carrying with it albumen, fibrin, blood-globules, and salts, is altogether disproved, both by actual analyses of the blood, and by the frequent occurrence of a jelly-like coagulum in the urine when no white fatty matter can be seen to be present.

3. The disease consists in some change in the kidney by which fibrin, albumen, blood-globules, and salts are allowed to pass out, whenever the circulation through the kidney is increased; and if at the same time fat is present in the blood, it escapes also into the urine. That this change of structure is not visible to the naked eye on *post-mortem* examination, Dr. Prout long since demonstrated; and in a case of this disease which was in St. George's Hospital, and was examined at Plymouth, no disease of the kidney was observed. From the total absence of fibrinous casts of the tubes from the urine, it is not improbable that by the microscope a difference may be detected in the structure of the mammary processes, rather than in that of the cortical part of the kidneys. —*Philosophical Transactions*, 1850.

9. *Remarks on the Cooking and Preserving of Meat.* By PROF. LIENIG.—The view that broth derives its nourishing properties essentially from the dissolved gelatin—an opinion which has frequently been discountenanced in practice—is shown by this investigation to be completely untenable. The gelatin imparts no taste to broth, and forms by far too insignificant a portion to allow of its

nutritious properties being dependent upon it. Chopped beef, or veal, previously exhausted in the cold, when boiled for five hours, yielded to the broth, the former 0.5 per cent. and the latter 1.5 per cent. of soluble constituents, of which gelatine formed, at most, but one-half. On the contrary, this investigation confirms the view of Prout, that the peculiar constituents of broth exist ready formed in the flesh, and are by no means merely products of the process of ebullition. The residue of the chopped muscular flesh of different animals—as of the fox and ox—after having been exhausted in the cold, cannot be distinguished the one from the other; all the peculiarities of the flesh, especially its flavour, depending entirely upon the soluble constituents which are found in the broth.

The researches of Lichig offer a simple and convenient method of preparing, in a few minutes, a broth of the highest nutritive properties. Finely-chopped lean beef is mixed with an equal weight of cold water, and left, if possible, to macerate for a short time, and the whole then slowly heated to ebullition. After gently boiling for some minutes, the clear broth separates from the coagulated albumen, and from the muscular fibre, which has now assumed a sinewy appearance. After straining, it requires only to be seasoned, and slightly coloured with burnt onions, or with caramel. The colouring of the broth is nothing but a concession to the common prejudice, which cannot, however, be well dispensed with. By evaporation in a water-bath, or at a still lower temperature, the broth becomes spontaneously coloured, and leaves behind a brown extract, possessing a delicate odour of roasted meat; this extract, when dissolved in about thirty parts of water, and flavoured with salt, yields, at any moment, a most excellent broth. The advantage of extract of flesh for the nutrition of invalids, its use in hospitals, or in field service, as well as in domestic economy, is sufficiently obvious. We see, likewise, that bone-broth, broth-tablets, &c., being preparations essentially different from a true broth from flesh, cannot enter into competition with it as articles of food.

As an article of commerce, extract of flesh bears somewhat too high a price. It appears, however, to offer a new source of profit to the inhabitants of the different settlements in America and Australia, who might successfully prepare it from their cattle at a cheaper rate, and send it to the markets of our crowded populations.

As to the cooking of meat, it follows that to prepare, by boiling, a rich broth, and, at the same time, a savoury *bouilli*, is perfectly impossible. After preparing broth to the above directions, the meat which remains is perfectly unpalatable, tasteless, and tough, and as dissimilar as possible to the boiled beef of our tables. If, on the other hand, it be desirable to leave in the boiled meat the greatest amount of nutrition and flavour, it must be at once plunged into boiling water. If the temperature, after some minutes, be reduced to about 158° Fahr. by the addition of cold water, and the water maintained at that temperature until the meat is thoroughly cooked, all the conditions necessary for this purpose will have been fulfilled. If it be perfectly established that pure fleshy fibre—viewed independently of the juice—instead of being softened by boiling, is converted into a horny or sinewy mass, it is evident that this change is prevented by two different means in the ordinary mode of cooking meat: in the first place, by the temperature in the interior of the piece of meat never reaching the boiling heat; and, in the second place, by its being, nevertheless, sufficiently high to coagulate the albumen which surrounds, and, to a certain extent, protects the fibre. The temperature in the interior of the meat is not only sufficient to coagulate the albumen (132° Fahr.), but must attain even the point necessary for the coagulation of the colouring matter of the blood (from 149° to 158° Fahr.).

The investigation of Liebig exhibits the process of salting meat under a perfectly new aspect: The "brine," which meat and dry salt form when together, amounts to from one-third to one-half of the juice of the meat, and contains the chief constituents of concentrated broth. The brine presents an acid reaction, and, owing to the quantity of albumen present, coagulates when boiled; it contains, moreover, phosphoric acid, lactic acid, a large amount of potassa, kreatinine, and, doubtlessly, also kreatine. There can be no doubt, therefore,



that salting diminishes the nutritious properties of meat, by the amount of constituents which pass into the brine; hence the explanation of the well-known injurious effect on health produced by the continued consumption of salt meat.—*Liebig's Report*, vol. ii.

10. *On the Oxidation of Ammonia in the Human Body, with some Remarks on Nitrification.* By H. B. JONES, M. D.—The author having shown, in a paper lately communicated to the Royal Society, that the effect of tartrate of ammonia on the acidity of the urine was totally different from that of tartrate of potash, and that carbonate of ammonia, taken in very large quantities, did not produce any alkaline reaction of the urine, but that, on the contrary, the acidity was rather increased than diminished by such doses, repeated the experiments with carbonate of ammonia, hoping to obtain more decided results. Although, from these experiments, it was again apparent that no diminution of the acid reaction resulted from taking carbonate of ammonia, yet the fact of any great increase in the acidity of the urine could not be determined. In his former paper, the author suggested that an inquiry into the occurrence of nitric acid in the urine would probably give the solution of this unexpected effect of carbonate of ammonia; and he was led to undertake the experiments described in the present paper with a view of detecting the presence of that acid under particular circumstances.

The indigo test for nitric acid being more delicate than the protosulphate of iron test, it was chiefly employed; but a mixture of starch, with a drop or two of solution of hydriodate of potash and hydrochloric acid, was found to be a far more delicate test than either. Beginning with 10 grs. of nitrate of potash added to 10 oz. of urine, it was found at last that as little as 1 gr. of nitre to 10 oz. of urine could be detected with the greatest certainty and clearness when the starch test was used; but this quantity could not be detected as surely by the indigo test.

Experiments are described in which carbonate of ammonia was given, in doses varying from 40 grs. to 7 grs., to a healthy man in whose urine no nitric acid could previously be detected; and the urine was tested at intervals of several hours after each dose. From these it appears that 10 grs. was the smallest quantity that gave decided evidence of nitric acid by both tests.

Having satisfied himself that when carbonate of ammonia was taken, small quantities of nitric acid passed off in the urine, the author made similar experiments with tartrate of ammonia, administered in doses of 60 and 40 grs.; and in each case the starch test gave evidence of the presence of nitric acid in the urine some hours after. Similar experiments with the muriate of ammonia are next described; and in these the presence of nitric acid in the urine was readily detected three hours after the administration of the dose, even when it was so small as 10 grs.

By an experiment described in the paper, it was shown that, by a simple combustion of ammonia out of the body, as well as in the body, nitric acid was produced. From other experiments it appears that urea, also, by oxidation, whether in the body or out of the body, gives rise to nitric acid.

Having found that nitric acid was produced more readily and frequently than had been supposed to be the case, the author was led to try whether combustions in the atmosphere without ammonia could not give nitric acid. The presence of this acid was, in consequence, detected in the products of the combustion of alcohol, of coal, of a wax candle, and of hydrogen.

As this led to the supposition that nitric acid might exist in rain-water at all times, experiments were made on the rain-water collected on wet days in London, and the presence of nitric acid was discovered by the starch and also by the indigo test.

The conclusions the author comes to from his experiments are:—

1. That the action of oxygen takes place in the body, not only on hydrogen, carbon, sulphur, and phosphorus, but also on nitrogen.
2. That in all cases of combustion, out of the body and in the body, if ammonia be present, it will be converted partly into nitric acid.

3. That the nitrogen of the air is not indifferent in ordinary cases of combustion, but that it gives rise to minute quantities of nitric acid.

He further remarks, that the production of nitric acid from ammonia in the body adds another to many instances of the action of oxygen in man; and that the detection of nitric acid in the urine may lead to the conclusion that the blood is being freed from ammonia, or from substances closely related to it, as urea, or possibly caffeine and other alkaloids.—*Monthly Journ. Med. Sci.*, May 1851.

## MATERIA MEDICA AND PHARMACY.

11. *New Experiments on the Woorara Poison.* By MM. CL. BERNARD and PELLOUZE.—It appears from these experiments, an account of which was read before the Academy of Sciences of Paris, that the action of the woorara is instantaneous when it is injected directly into the vessels. When the jugular vein of dogs and rabbits has been thus injected, the animals have died as if struck with lightning, without screams or convulsions. If the woorara is introduced under the skin in solid fragments or in solution, its action is not so rapid, and in proportion with the size of the animal and the dose. Birds and mammals die in a few minutes, but it takes several hours to destroy a reptile with the poison. Birds and dogs do not seem immediately affected after the puncture: the former fly for a little while, but soon fall and die without a struggle; the latter walk about at first, then lie down as if tired, and seem to fall asleep and die imperceptibly. Galvanism has, after death, no influence on the nerves, the blood is black, refuses to coagulate, and is no longer reddened by the contact of the air.

The woorara may without danger be ingested into the digestive canal, but if the poison be mixed with gastric juice, and introduced under the skin, it takes effect. Thus it would appear that the woorara is not decomposed in the stomach, it is merely dissolved in that organ, but the mucous membrane does not absorb it. A simple experiment will prove that this property of mucous membranes is of a purely physical and not a vital nature. If a piece of gastrointestinal mucous membrane is adapted to a glass tube, with the mucous surface turned outwardly, a watery solution of sugar having been introduced into the tube, the liquid will rise in the tube if it be plunged into a solution of woorara, endosmosis will have taken place, but no woorara has traversed the membrane. The poison will, however, penetrate the membrane if the latter be not fresh, and the contact last very long.—*Lancet* 15th March, 1851.

12. *Preparation of Atropine by means of Chloroform.*—The following method of employing chloroform for the purpose of separating organic alkalies has been proposed by M. RANOVANIX, of Orleans:—

The expressed juice of the fresh belladonna is beaten until the albumen coagulates, and then filtered. To the cold, clear fluid are added 4 scruples of caustic potash and 30 scruples of chloroform per pint; the whole is then agitated for a minute and allowed to depose. In half an hour the chloroform depose, charged with atropine, under the appearance of a greenish oil. The supernatant fluid is now decanted and replaced by some water; the latter is also decanted, and the operation repeated until the water comes off limpid. The solution of atropine in chloroform is now distilled in a sand-bath, until all the chloroform has passed off, and the residue is dissolved in a weak solution of sulphuric acid, which takes up the atropine. To obtain the latter in a perfectly pure state, we add carbonate of potash in excess, collect the residue, dissolve it in alcohol, and evaporate the latter.—*Med. Times*, May 24th, 1851, from the *Bulletin de l'Institut*.

## MEDICAL PATHOLOGY AND THERAPEUTICS AND PRACTICAL MEDICINE.

13. *Value of Vaccination.*—[Dr. GUY, in his interesting lectures on public health, now in course of publication in the *Medical Times*, has brought forward the following striking statements illustrative of the great value of vaccination, and of the vast saving of human life and human suffering it has been the means of effecting:—]

In the history of small-pox, we have three different periods to compare with each other: a period anterior to the introduction of inoculation; a period during which inoculation was practiced; and a period during which inoculation was superseded by vaccination. As inoculation was introduced into England in 1721, the deaths from small-pox during the ten years ending 1719 will fairly represent the mortality occasioned by small-pox, unmodified by the practice of inoculation. Now, as I have already stated, the practice of inoculation did not gain much ground till towards the middle of the 18th century, and did not become general in England till the last quarter of that century. Hence the ten years from 1740 to 1749, inclusive, will correspond to the introduction of inoculation into partial use; and the ten years from 1790 to 1799, inclusive, will mark the time when it was largely and generally practiced. In like manner, the ten years from 1810 to 1819, inclusive, will represent a period during which inoculation was to a great extent, though by no means altogether, superseded by vaccination. The ten years from 1840 to 1849, inclusive, will represent a period when small-pox inoculation, having fallen into entire disuse, and been even declared an illegal practice, vaccination has come to be the only preventive of small-pox employed or permitted. It must not, however, be forgotten, that large numbers of persons still remain unvaccinated, though the greatest possible facilities are offered for the performance of the operation, and that without charge to the poor.

I have arranged the deaths by small-pox for the five periods of ten years each to which I have just referred, in a tabular form, so that the numbers may admit of easy comparison.

	Deaths from Small-pox.	Estimated Population within the Bills of Mortality (limits in 1725.)	Deaths from Small-pox in a million inhabitants.
1. Ten years ending 1719, before in- oculation .....	21,223	675,691	31,416
2. Ten years ending 1749, inoculation partially practiced	20,029	708,188	28,282
3. Ten years ending 1799, inoculation in general use ..	17,635	773,344	22,863
4. Ten years ending 1819, vaccination in general use ..	8,334	1,035,865	8,045
5. Ten years ending 1849, inoculation superseded by vac- cination (whole metropolis) .....	9,174	1,912,172	4,798

A small decrease in the number of deaths from small-pox, coincident with the partial practice of inoculation; a still more marked decrease under the more general use of that palliative; but a far more remarkable falling off in the number of deaths from small-pox, concurrently with the introduction and extension of vaccination: such are the results stamped on the very face of the table which you have before you,—results fully borne out by Mr. Farr, who says that, "In 1771 to 1780, not less than 5 in 1000 died annually from small-

pox; while in 1801 to 1810, the mortality sank to 2; and in 1831 to 1835, to 0.83," or less than 1 in 1000.

The decrease in the number of deaths from small-pox in the second and third periods of ten years, is a circumstance in favour of the views of the supporters of inoculation, who affirm that that practice, though objectionable in one point of view, was on the whole beneficial.

That inoculation may really have been the means of saving life to some extent does not seem altogether improbable, when we bear in mind that in place of the mortality of *one in four*, which attended the small-pox caught in the usual way, the loss under inoculation, when skilfully and carefully practiced, did not exceed *one in 500*. So that it is quite possible that the mischief which inoculation wrought by spreading the disease among those who might never have caught the natural small-pox, was counteracted by the good it effected in reducing the danger to a very insignificant amount. But even admitting the justice of this compensation, the benefit really due to inoculation does not exceed the saving of 8500 lives in 31,500; whereas the boon conferred by vaccination is represented by a reduction of the mortality from nearly 23,000 in the ten years ending 1799 (the very heyday of inoculation) to 8000, and then to less than 5000, being a saving of life more than twice as great as that effected by inoculation.

But the superiority of vaccination over inoculation is only partially and imperfectly represented by the greater saving of life which it effects. It has the great additional merit of requiring no preparation or confinement, of producing very trifling indisposition, and of entailing no danger. Above all, it is free from the great objection to inoculation, that, while it mitigates the dissonance in the individual, it brings the infection home to those who may happen to be unprotected, and who might never have been exposed to the contagion of the natural small-pox.

Taking all these considerations into the account, there can be no doubt that the legislature was perfectly justified in rendering the performance of inoculation a legal offence, and in giving, by the appointment of public vaccinators, to be remunerated by a moderate fixed charge, large facilities for carrying the blessing of vaccination to the very doors of the poor.

But, undisputed as is the superiority of vaccination, and undoubted as are the benefits it has conferred, they are small indeed compared to those which it was calculated to bestow if the practice of it had been as universal as it ought to have been. It was the cherished opinion of Jenner himself that we possess in vaccination a means of entirely eradicating the small-pox, and some facts are on record which seem fully to bear him out in that view of the case. Thus, Sir Gilbert Blane tells us that he was informed, on good authority, "that vaccination had been practiced with so much energy and success in Spain that for the last twelve months there had occurred not only no deaths from, but no cases of small-pox; that the new-born children of all ranks are carried as regularly to the vaccination-house as to the font of baptism; that the small-pox is entirely extinguished all over Peru, nearly so in Chili, and that there has been no compulsory interference on the part of the Government to promote vaccination." Sir Gilbert Blane goes on to say, "It is now matter of irrefragable historical evidence, that vaccination possesses powers adequate to the great end proposed by its meritorious discoverer in his first promulgation of it in 1798, namely, the total extirpation of small-pox. The first proof of this was at Vienna, where, in 1804, no cases occurred except two strangers, who came into the city with the disease upon them. In 1805, there did not occur a single death from it in Copenhagen."

I quote this more detailed account of the progress of the small-pox in Denmark from a MS. of Jenner's, published by Dr. Baron in his *Life of Jenner*. "From the year 1762 to 1792, the number that died of small-pox in the Danish dominions amounted to 9728. About the year 1802, vaccination was first introduced, and the practice became general, but not universal: however, fifty-eight persons only died of the small-pox to the year 1810. Vaccination, by command of the king, was now universally adopted, and small-pox inoculation prohibited. And from the year 1810 to the year 1819, not a single case of

small-pox has occurred." From the same MS. I quote the following passage: "From Bombay, I learn the small-pox is there completely subdued, not a single case having occurred for the last two years."

"Dr. Sacco, the indefatigable superintendent of vaccination, in Lombardy (I resume my quotation from Sir Gilbert Blane), stated, in his annual report, 3d of Jan. 1808, that small-pox had entirely disappeared in all the large towns in that country; and that in the great city of Milan it had not appeared for several years. Dr. Odier, of Geneva, so favourably known for his high professional, scientific, and literary acquirements, testifies that, after a vigorous perseverance in vaccination for six years, the small-pox had disappeared in that city and the whole surrounding districts, and that, when casually introduced by strangers, it did not spread, the inhabitants not being susceptible. The central committee in Paris testify, in their report of 1809, that the small-pox had been extinguished at Lyons and other districts of France." "These (I still quote from Sir Gilbert Blane) are selected as some of the earliest and most remarkable proofs of the extirpating power. But it is demonstrable that if, at the first moment of this singular discovery, at any moment since, at the present or any future moment, mankind were sufficiently wise and decided to vaccinate the whole of the human species who have not gone through the small-pox, from that moment would this most loathsome and afflicting of all the scourges of humanity be instantaneously and forever banished from the earth." If for this somewhat too enthusiastic view of the case, we substitute (what Sir Gilbert Blane probably intended to affirm) that if in any way it could be brought about that every man, woman, and child, for a term of years, could be vaccinated, small-pox would gradually die out, and ultimately disappear, like a fire extinguished for want of fuel, we should be stating what all experience and analogy seem to warrant. In our own country, we have not, that I am aware, had any proof afforded us of the extirpating power of vaccination. But we have had some striking and encouraging illustrations of the power of vaccination when systematically practiced. The army affords us one such instance. In Her Majesty's dragoon guards and dragoon regiments, which, in common with the rest of the army, are submitted to inspection, and vaccinated without exception, if that operation is found to have been omitted, "the deaths from small-pox during a period of seven and a quarter years, in an average annual strength of 6165 men, were only *three*—a proportion which would represent an annual mortality of 1 in 14,900 adult males!"

The value of vaccination is shown in a very remarkable manner by the experience of certain severe epidemics of small-pox, which have taken place since vaccination came into general use in this country. There was such an epidemic in Scotland in 1818-1819, when a careful analysis of the cases showed that, out of 205 cases occurring in persons unprotected either by vaccination or previous small-pox, 50 died, being one death in every *four* cases; out of 71 cases in which small-pox had occurred a second time, there were *three* deaths, or one in 23; while out of 310 cases, occurring after vaccination, one only died! In this instance, then, the protecting power of vaccination proved to be more than 13 times as great as the protecting power of small-pox itself. A similar result was established in the epidemic of small-pox which occurred at Marseilles in 1828. It was estimated that the population consisted of 8000 *unprotected* persons; of 2000 who had been *inoculated*, and of 30,000 who had been *vaccinated*. Now, of these 8000 unprotected persons, 4000, or one-half, were attacked by small-pox; of the 2000 who had been inoculated, 20 were attacked, being 1 in 100; and of the 30,000 who had been vaccinated, 2000, or 1 in 15, were attacked. The mortality of the several classes was as follows: Of the 4000 unprotected persons, 1000, or 1 in 4, died; of the 20 inoculated persons, 4, or 1 in 5; of the 2000 vaccinated persons, 20, or 1 in 1000! It would seem, then, that the practice of vaccination is less effectual in preventing small-pox than that of inoculation; but that, at the same time, the disease, when it does occur after vaccination, is of so mild a character as to destroy only 1 life for every 20 sacrificed by small-pox occurring after inoculation.

The relative value of vaccination and inoculation, and the proportional risk of the protected and unprotected, will be best seen by supposing 15,000 unpro-

tested, inoculated, and vaccinated persons, respectively, to be exposed to the danger of catching small-pox. The result as regards these several parties are shown in the annexed table:—

	Unprotected.	Inoculated.	Vaccinated.
	15000	15000	15000
Attacks .....	7500	150	1000
Deaths .....	1875	30	10
Ratio of attacks	1 in 2	1 in 100	1 in 15
Ratio of deaths to attacks ....	1 in 4	1 in 5	1 in 100

In order to form a just idea of the superiority of vaccination, we must not forget to add to the lives saved by it, the blindness, deformity, and lingering debility which it is the means of preventing in those cases which do not prove fatal; for it may safely be assumed that these untoward consequences bear a certain relation to the fatality of the disease, being of more frequent occurrence where the mortality is highest.

14. *Case of Small-Pox recurring a third time after Vaccination, when it proved fatal.*—Dr. JOHN WEBSTER communicated an example of this to the Royal Medical and Chirurgical Society, Feb. 26, 1851. After alluding to the fact that whooping-cough, measles, and scarlatina generally occur only once during the lifetime of an individual, exceptions, nevertheless, to the above rule, as well in these complaints as in small-pox, have been recorded by authors. Three well-marked examples of the recurrence of small-pox met with in the same family are related, one of which terminated fatally. The case especially referred to by Dr. Webster was that of H. N. N—, who had been vaccinated satisfactorily in 1827, when three months old. Notwithstanding this circumstance, he became attacked by small-pox in 1833, along with an elder brother, who had been likewise vaccinated. Both patients recovered, and nothing more was thought of the matter till 1838, when the two lads were again attacked by variola, along with another—that is, a third—brother, likewise regularly vaccinated. However, all three got quite well in due time. Subsequently, Mr. H. N. N—, whose case is now just mentioned, went to India in the Company's service, where he was seized, in April last, with the usual and well-marked symptoms of small-pox, which soon became confluent, and proved fatal at Dharwarinth, on the 13th of that month; this making the third time this gentleman had been attacked by variola, although previously vaccinated.

15. *Case of Confluent Small-Pox after a third Vaccination.*—The following case of this kind was communicated to the Royal Medical and Chirurgical Society by Dr. A. P. STEWART. A gentleman twenty-five years of age, who had been five years apprenticed to a gentleman in extensive general practice in Devonshire, had been most successfully vaccinated when six months old. Requiring to attend variolous cases, he had the operation repeated in July, 1849, without effect, and again in May, 1850, with much irritation, inflammation, and swelling of the arm, the lymph from the vesicle not communicating the disease. He came to London to begin his medical studies on the 13th of October last, having been exposed to infection the same morning. The train of premonitory symptoms set in on the 15th, and was followed (on the 17th) by the appearance of a closely packed eruption of papulæ all over the body. For four successive nights, mild delirium was present; but the sore throat and conjunctivitis occasioned by the presence of vesicles were quickly relieved by the application of a solution of nitrate of silver; and the pulse, previously as high as 120, never rose above 100 after the seventh day. The vesicles, though very numerous on the hands and feet, caused little or no pain; on the eleventh day of disease, and seventh day of eruption, they were at their height, and in the course of the next two days, had nearly all dried up and scaled off, with very trifling discharge. Thereafter convalescence went on rapidly. In the district where the subject of this attack resided, the popular prejudice against vaccination has always been so strong that for a number of years it had been much neg-

lected; and the parents are still in the habit of inoculating their children. During the last nine months, it has been practiced to a very large extent, with so much secrecy as to defy the utmost efforts of the authorities to obtain legal proof, and with the effect of keeping up the disease during many months, and spreading it abroad over the whole district. It was introduced in April by a sweep boy, who took it in a town not far off, and, returning to his family, communicated it to his unvaccinated brothers and sisters. The latter, again, going to a lace school, infected several of the other children, after which it spread rapidly in all directions. Several instances were adduced, as examples of a large number in which inoculation had entirely failed to protect those who had been subjected to it, from secondary attacks; besides the case of a lady of title, whose mansion was in the infected district, and who had lately had a second attack of natural small-pox. The author then stated, as the result of his and Dr. A. Anderson's experience in the Glasgow Fever Hospital, from November, 1836, to November, 1838, that of the 126 cases admitted, 31 were vaccinated, 52 doubtful (consisting of unvaccinated, with a sprinkling of imperfectly vaccinated patients), and 43 unvaccinated. The mortality among the "doubtful" was 1 in 3.06, or 32.7 per cent.; among the unvaccinated, 1 in 2.86, or 34.8 per cent.; while among the vaccinated it was only 1 in 31, or 3.2 per cent. The author concluded his remarks by adducing his experience in the north-west district of St. Paneras parish, during the epidemic of 1845. Those who had been successfully vaccinated, both children and parents, though exposed night and day, in their unventilated dwellings, to the concentrated infection of the disease in its worst forms, were proof against it; while those in whom the evidence of success was doubtful, were not proof against the milder forms, and very often took the disease from convalescents whom they met in the open air. The author concluded, with Chomel, that "we cannot fairly exact more from vaccination than from the small-pox itself."

16. *Protective Power of Vaccination.*—[The reading of the two preceding cases before the Royal Medical and Chirurgical Society gave rise to an extremely interesting discussion, during which, Dr. Gregory made some statements of great importance, and the correctness of which is well worthy of investigation. We subjoin the report of the discussion as given in the *Lancet*, March 8:—]

Dr. GREGORY said that the cases detailed were interesting, and contained much which was worthy of the consideration of the Society. With respect to the first case, in which a person contracted small-pox in advanced life after vaccination, and died in India, he might remark, that the disease was more fatal in warm climates, and the case in this respect was not remarkable. The similarity of the cases, however, consisted in the statement which had been made, that these patients had small-pox three years after, and again five years after vaccination. Now, he would beg to ask Dr. Webster, who attended these cases, was he acquainted with the medical man who had the patients under his charge? He asked this, in order that it might be determined if the disease under which these patients laboured was variola or not. Certain it was that he (Dr. Gregory) never saw such cases; they never presented themselves at the Small-Pox Hospital. He must, therefore, be excused from doubting whether these cases were really cases of variola, until the doubt had been cleared up by further information from Dr. Webster. With respect to the second case, which was one of confluent small-pox after vaccination, it really contained nothing remarkable, except, indeed, the fact of re-vaccination having been practiced and followed by local inflammation. This showed convincingly the little value of re-vaccination. He had held this opinion for a long time past. But by far the most important part of the paper was its closing paragraph, the doctrine of Chomel, "that we have no right to expect more from vaccination than from inoculation itself." These few words involved considerations of the highest importance, and it was certain that the attention of the profession and the public must be called to the entire question, and to determine the value of the dictum in question. What, for instance, did we expect from inoculation? By it we gave the small-pox to the person inoculated. We submitted the system to a subtle and violent miasma; the system fortunately took it mildly, passed

through the disease, and thus the matter ended. There might be exceptional cases to this termination, and some might have the ill luck to have small-pox a second time; but he believed few entertained fear on this point. No one above fifty who had been inoculated, he believed, had the slightest fear of small-pox. He had certainly seen cases in which this had occurred, but the chances in favour of it were very small indeed. But did the facts with which we were acquainted warrant us in banishing this fear when vaccination had been resorted to? He believed not; facts did not warrant us in banishing such fear. Up to puberty we might banish it. Cases of small-pox after vaccination up to the fifteenth year were very rare indeed; but after that period vaccinated persons were exposed to the chances of a first attack of small-pox, and were then liable to a second attack of that disease at fifty or sixty years of age. Thus inoculation was to give one attack of small-pox and have done with it; vaccination was to do away with the disease altogether, as it did up to puberty, but after that the patient was liable to the disease. Now, we never heard of modified small-pox up to the year 1817, or about fifteen years after vaccination was first performed. These cases of this disease had gone on increasing since the year 1825, and the results now were, that fifteen hundred patients had been admitted into the Small-Pox Hospital in seven years, with small-pox after vaccination; most of these were modified, but many were of a severe form of the disease. The deaths from small-pox after vaccination might be said to be a little above five per cent.; occasionally in some places, as in Copenhagen, for instance, it was as low as three per cent.; but taking the entire world it would amount to five per cent.

Dr. Mayo believed that, in the times previous to the epidemic to which he had referred, a great number of persons had been inoculated, that a greater number became affected with small-pox after inoculation than after vaccination, and that more of the first class than of the second died. If Dr. Gregory remembered the work to which he (Dr. Mayo) had referred, he begged to inquire into this point.

Dr. WEBSTER said, two observations which had fallen from Dr. Gregory were so important that he must reply to them thus early. The first regarded the authenticity of the cases now brought before the Society, and whether they were genuine small-pox. On this point no doubt could prevail, as they were all attended by medical practitioners, who, as well as the parents, entertained no hesitation respecting the nature of the disease; whilst, in the particular case terminating fatally in India, the statement was made on the authority of the regimental surgeon who attended the patient during his last and fatal attack of variola. Dr. Gregory had likewise stated that small-pox was generally more severe in warm climates than in England. This opinion was quite correct, and fully borne out by the case just related; hence, persons imperfectly vaccinated, and therefore unprotected, ran greater risks in such countries than elsewhere. In proof of this peculiarity, he would beg leave to recall to the recollection of the president an interesting case, which had come under his own immediate observation, in which the patient, although vaccinated when young, fell ill of small-pox during the time he was a student at Guy's Hospital. From this attack the gentleman in question recovered by the president's judicious treatment; but, having subsequently gone to India, he there fell a sacrifice to that complaint. Another illustration of the same kind had also been mentioned to him (Dr. Webster) by Dr. McIntyre, which merited record. It was that of an officer, likewise serving in the East India Company's service, who became attacked by malignant variola, in the north-western provinces of Bengal, from which he died, although previously properly vaccinated in England. The brother of this individual, then travelling in South America, where small-pox prevailed, although both vaccinated and inoculated when young, got so alarmed and doubtful respecting his immunity from attacks of the above disease, that he was again vaccinated, after hearing of his relative's death in India. Notwithstanding this additional precaution, secondary small-pox, of a virulent sort, manifested itself, from which the patient would have died, according to his medical attendant's opinion, but for the recent vaccination. This and the other cases further illustrated, in a marked manner, the predisposition to receive the



variolous poison more than once by members of certain families, especially in the examples narrated in his (Dr. Webster's) communication, although they were all vaccinated and also had small-pox previously. It was also curious that, in every instance, the males only were affected, whilst the sister of the three brothers just mentioned was not attacked by the malady. In bringing forward the present anomalous cases, it was not his (Dr. Webster's) intention to detract from the great protective benefits of cow-pox, but simply to record an illustration of a most unusual occurrence; and he would add, no one entertained a higher opinion of the benefits following vaccination, when properly performed, than himself, which seemed to be conclusively proved by the fact that, previous to the introduction of inoculation, one-tenth of the total mortality in England was occasioned by small-pox. After inoculation was introduced, it fell to one-fourteenth; but now, when cow-pox has become more generally diffused than some years ago, the deaths by variola are nearly one in eighty-five from all diseases: thus amounting to only one-sixth of the ratio when variolous inoculation was the sole preventive.

Dr. HALLÉY begged to ask Dr. Gregory his opinion on the following point: If the protective power of vaccination lasted only to the fifteenth year, would it be advisable to re-vaccinate persons at that period?

Dr. GREGORY had not stated that the protective power of vaccination ceased at fifteen, but that, up to that period, the protection afforded by it was as great as that afforded by inoculation all through life. After fifteen, the system was subjected to another law. With respect to the question, as to whether re-vaccination after fifteen renewed the protection, he might answer that, in his opinion, re-vaccination was a proceeding of very little moment. It satisfied the mind of the public, rather than effected any real good. If it satisfied the mind, why should it not be performed? Let it be so, by all manner of means; but not under the impression that it afforded any protection. Well, then, what was the law which prevailed with respect to the influence of vaccination after fifteen? The constitution, at that period, began to be susceptible, for the first time, to the influence of small-pox, and the susceptibility increased up to middle age and maturity.

We had, however, he thought, means of increasing the protective power of the first vaccination by inoculation after the age of fifteen. The statute-law, in reference to inoculation in this country, prohibited experiments on this point. In France, another law on the subject of inoculation prevailed, not having the same object in view as in this country. Cazenave and others had performed many experiments on this point, which proved that inoculation after fifteen, in persons previously vaccinated, did not produce a vesicular or pustular eruption, but only a papular one, and that this was not contagious. This he knew to be true, and he firmly believed it acted as a protection for life. Several Italian physicians had urgently requested permission from the Austrian government to follow out the experiments of Cazenave; but the authorities refused their sanction to the proceedings, though, in the Austrian dominions, the prohibition was by edict, and not by statute; hence the hardship of non-compliance with the request was greater.

Dr. COPLAND said his experience went to prove the correctness of Dr. Mayo's statement, respecting the protective power of vaccination until the age of fourteen being more complete than that of inoculation, and that the proportion of persons affected with small-pox was less from the former than the latter. When he commenced practice, he attended a family at Hockney, some of whom had come under his care with small-pox after vaccination: the vaccination had been properly performed, and the cicatrices were distinct and complete. The grown up persons in the family had the disease very badly, and it became more and more modified as the ages of the patients decreased; so that in a boy of eleven years of age it assumed a nearly papular form. From these and other facts which had come to his knowledge at the time, he arrived at the conclusion that vaccination exerted only temporary protection from small-pox, and that it was not, as advanced at the time, a protection for life. This had since been proved to be correct. Dr. Copland then referred to a case of fatal malignant small-pox occurring in a lady who had been vaccinated twenty years before, in

support of the opinion that the protective power of vaccination did not extend beyond puberty.

Dr. BASHAM thought that efficient stress had not been laid upon the distinction which he thought ought to be drawn between perfect and imperfect vaccination. Many of the latter he believed were those who were affected with small-pox. The other day he had taken ten patients at random in the Westminster Hospital, and, on carefully examining their arms, had found only four which presented the true fixed and characteristic cicatrix of perfect vaccination: the other six were suppurative and hasty scars, and insufficient to determine that vaccination had been properly performed.

Dr. SNOW said that he had attended a case of modified small-pox after vaccination in a child three years of age, and had seen other cases of modified small-pox in children—nearly as many, in fact, as he had seen in the adult. He would be glad to know from Dr. Gregory an what data he founded his opinion that vaccination was an almost complete protection up to about the age of fifteen, and a much worse protection in after life. If this data were founded on the experience of the Small-Pox Hospital, he would suggest that it might be open to fallacy; for modified small-pox in children was so mild a disorder, that mothers would not think it necessary to send their children to an hospital on account of it; but when it occurred about the period of puberty, the subjects of it were generally away from their mothers, in situations where it was inconvenient to attend to them, and amongst persons who had a great dread of contagion, and the patients were consequently sent to the Small-Pox Hospital. We very seldom saw small-pox in persons of thirty-five, forty, or forty-five years of age, although the majority of people now living at that period of life depended for their protection on vaccination, and not on inoculation. The great body of practicing medical men, for instance, had been vaccinated—not inoculated; and although they were every now and then called to a case of small-pox, they rarely took the disease. He could not, therefore, concur in the fears of Dr. Copland with respect to future epidemics of small-pox.

Dr. WEBSTER remarked, as allusion had been made to Dr. Thompson's statistics of the mortality in small-pox occurring after vaccination, he might mention, amongst 71 cases of variola met with by that author, in persons previously vaccinated, only three died, or one in 24 examples; whereas, in 205 cases of primary small-pox, 50 died, being one death in every four patients attacked. Indeed, a great proportion of the mortality from variola then occurred, as at present, amongst unprotected persons. For instance, during the last quarter, 69 individuals died at Nilston from small-pox, of whom 50 were not vaccinated. In the eastern division of Wolverhampton, amongst 61 deaths from the same disease, 46 never had undergone vaccination. In Dudley, where 60 persons likewise died from variola, 50 of these never were vaccinated; whilst at Coventry, out of 65 deaths by the same cause, only eight of the entire number were said to have had cow-pox previously. But such occurrences were not surprising, considering the prejudices still prevalent in many parts of the country respecting the utility of cow-pox. Some parents objected to vaccination, because it was giving the disease of an animal to their children; others, because it was flying in the face of Providence to employ such means to prevent the spreading of a natural complaint; whilst some disliked novelty, and preferred inoculation. He (Dr. Webster) must again repeat, notwithstanding vaccination was not invariably a preventive of variola, it nevertheless had conferred immense benefits upon the community, and should be constantly employed; of course, taking due care that genuine cow-pox was always communicated. To prove how much good this modern discovery had accomplished, it was only necessary to look at the large assemblage of smooth faces now in that room, where not a single individual marked by small-pox could be seen. (A laugh.) Again, when walking in the crowded streets of London, it was equally rare to meet instances of the kind, thus shewing a very different state of matters from that formerly prevalent.

Dr. GREGORY, in reference to Dr. Basham's remark respecting the indications afforded by the appearance of the cicatrix as to the perfect or the imperfect performance of vaccination, said that he thought it had been long ago conclusively

settled that no conclusion whatever could be drawn, with respect to this point, from the appearance of the cicatrix. If a good cicatrix were found, then you might be satisfied that the vaccination had been perfectly performed; but if the cicatrix were imperfect, you had no right to assume that the patient had not been well vaccinated; for in these cases the process of reparation might have been quick; there might have been little inflammation, or there might be other causes to account for the imperfect cicatrix. He had long ago published cases on this point. Dr. Snow had asked him on what evidence he had arrived at the conclusion that vaccination afforded nearly complete protection from small-pox up to the period of puberty? and had inferred that he had derived his evidence mainly from the small-pox hospital; but this was not the fact. He had drawn his conclusions from extensive observations, and from correspondence with persons all over the kingdom. He had never seen a modified small-pox in this young, either in private or public practice; neither had he read of it in books, or heard of its occurrence abroad. It was only to be found occurring in the adult. Dr. Webster had expressed his hope that the facts and statements advanced would not shake the confidence of the public in vaccination. He (Dr. Gregory) hoped so, too. Even if vaccination prevented small-pox in only one-half of the cases in which it was performed, it was a great protection; and as it was shown by statistics that about half the population died before the age of fifteen, it afforded to that half, at least, perfect and complete protection.

Mr. ANNOTT inquired of Dr. Gregory what was the relative mortality from cases of small-pox without, and after, vaccination?

Dr. GREGORY replied that, from the year 1844 to 1850, 2854 cases of small-pox had been admitted into the hospital: of these 1500 were after vaccination. The deaths among the entire number had been 579: of these, only 75 were amongst the vaccinated portion. It was a simple matter of calculation to determine the relative mortality.

Dr. ANNISON inquired of Dr. Gregory what his impression was respecting the identity of small-pox and chicken-pox?

Dr. MARSHALL HALL rose and said that he was just about to ask Dr. Gregory the same question—viz., his opinion of Dr. Thompson's view of the identity of vaccinia and the modified small-pox? There was another question which he begged to ask Dr. Gregory. It sometimes happened that a child resisted vaccination. Was it then liable to take the small-pox? His own son had been vaccinated fourteen times unavailingly—once by his friend Mr. Barlow, who sat near him. No vaccine vesicle ever formed. His son was sent to Eton, being thirteen years of age; and on coming home, on July the 29th, 1844, for the holidays, went to Brighton. There, on August the 12th, he was observed to be covered with an eruption. Some of the spots at once exhibited the form of distended vesicles, of moderate size, observed in *chicken-pox*. Others of the spots went through the regular course of *horn-pock*, occupying five or six days. One or two on the face left distinct *pits*, the result of sloughing, as seen in *small-pox*. Dr. Hall added, that such a case seemed to demonstrate the insecurity of the patient, when vaccination had failed several times, and to confirm the opinion of Dr. Thompson, that variella and modified small-pox were the same disease, for in it they occurred simultaneously. Some children seem to possess peculiarities in regard to their susceptibility to the eruptive diseases. The same boy, on returning to Eton, took the scarlatina. There was, however, no rash on the skin; but the sore-throat and the enlarged papillæ of the tongue were so distinctly scarlatinaous, that he (Dr. Hall) had had a drawing made, which he still preserved, as admirably representing this form of eruption.

Dr. GREGORY, in answer to the question respecting the identity of small-pox and chicken-pox, replied, that the diseases, though bearing some relation to each other, were undoubtedly different and distinct in their nature. In proof of this it had been demonstrated that genuine vaccination had been received before and after the occurrence of chicken-pox. The occurrence of the latter previously made no difference to the development of the former. In addition to this, the two diseases might go on together in the same person. Now, in reference to small-pox, we never saw these modifications. With respect to the case mentioned by Dr. Hall, he had no hesitation in saying that the variolous

miasma had done its worst; and though, like the patients mentioned in the paper of Dr. Webster, Dr. Hall's son might have the ill luck to contract secondary small-pox, the great probability was that he would not.

Dr. A. P. STEWART said that his object in bringing his paper before the Society was to show the influence of vaccination on a case of modified confluent small-pox, in opposition to the case of Dr. Webster, in which death from small-pox had followed vaccination. He had an additional object, however, in his paper, and that was, to draw attention to the remarkable epidemic which had occurred in Devonshire, to mention the strong prejudice which existed in that locality against vaccination, and the bad effects which had resulted from inoculation for variola—a practice which could only be justified by knowing that it afforded a real, effective, and permanent protection from a second attack of small-pox. Dr. Mayo had appealed to the experience of Dr. Thompson on this point, and the results in Devonshire fully bore out that experience; for in that locality the persons inoculated were affected by secondary small-pox in a larger ratio than those who were vaccinated. There was no question that a larger number of these persons were inoculated, and the statistics showed that they were not protected from a second attack of the disease. He regarded the occurrence of 1500 cases of small-pox after vaccination as a very small number indeed, out of a population of two or three millions, and that fact alone showed the great protective power of vaccination. But it was not in hospitals that the protective power was to be seen in its full force: in those institutions the more striking illustrations were not witnessed; but in the poor districts, where day after day the small-pox patients were crowded in confined, close, and unventilated rooms, the medical man in attendance had every opportunity of seeing the vaccinated mixing with the infected persons, and remaining free from the influence of the contagion. No stronger proofs than these of the protective power of vaccination could possibly be adduced. (Hear! hear!)

17. *Observations on the Urine in Pneumonia.*—Dr. REDTENBACHER was engaged, in the year 1846, in making a series of experiments on the urine of patients labouring under different acute and chronic diseases, with the view of determining—first, the relation which subsists between the presence of inorganic salts and the specific gravity of the urine—and secondly, whether the increased or diminished proportion of these saline constituents is connected with the formation or retrocession of certain morbid processes. His observations were made in Skoda's wards in the Great General Hospital of Vienna. He found that the amount of inorganic salts was not subject to much variation, and consequently affected the density of the urine in a very trifling degree; but that its density depends upon the inorganic compounds, and especially upon the urea and uric acid. In cases of pneumonia, he has satisfied himself that the amount of chlorides in the urine undergoes variations dependent upon the stage of the disease. Healthy urine, as is well known, deposits a copious precipitate on the addition of nitrate of silver. Redtenbacher asserts that in all the cases of pneumonia (amounting to 80) in which he has applied this test, the faintness or absence of the precipitate has indicated a diminution or absence of chlorides. The precipitate becomes less distinct, from day to day, till resolution commences, when the chlorides again gradually resume their normal proportion. Diminution of the chlorides likewise occurs in typhoid fever, in acute arthritis, and in capillary bronchitis; but is neither a constant phenomenon in these diseases, nor observed at any certain period of the morbid processes. In cases of tubercular infiltration chlorides are found in the urine in normal proportion, but suffer diminution for a short time during each fresh infiltration.

Redtenbacher anticipates the objection as to the value of these observations—that the amount of the chlorides of the urine depends upon the amount of culinary salt used with the food, and that the quantity of the former will be diminished with the supply of the latter—first, by showing that the chlorides are not equally diminished in the course of other diseases, in which restricted diet is rigidly enforced; and secondly, by mentioning that Skoda's cases of pneumonia, on which his observations were made, were, from

beginning to end of the disease, allowed a drink acidulated with muriatic acid. He believes, consequently, that the uniform diminution of chlorides in the urine of pneumonia, is somehow essentially connected with the morbid process.—*Monthly Journ. Med. Sci.*, April, 1851, from *Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien*, Aug. 1850.

18. *On Exaltation of Hearing in Paralysis of the Facial Nerve.*—The Nos. of the *Gazette Médicale de Paris*, for Feb. 8th and 15th, contain a memoir on this subject by Prof. LANDOUZY, of Rheims. The following are his conclusions:—

1st. In its pathological relation the exaltation of hearing on the paralyzed side is an almost constant symptom of facial hemiplegia, independent of all cerebral affection.

2d. This exaltation appears at the same time with the hemiplegia, and disappears before it.

3d. It is to be attributed to the paralysis of the internal muscle of the malleus.

4th. It indicates that the nervous lesion is not situated under the first bend of the seventh pair.

5th. It may exist in the absence of the facial hemiplegia.

6th. That it may either coexist with hemiplegia, or be independent of it; it disappears spontaneously, entirely, and in the course of a fortnight or three months.

7th. To decide its existence, it is sometimes necessary to act on the hearing by a loud noise more intense in proportion to the time that has elapsed since the commencement of the affection.

8th. Special treatment will nearly always be useless; where it becomes necessary, it consists in tamponing the ear on the paralyzed side, and even both sides, to diminish the action of sonorous waves, to direct with prudence some cold or slightly astringent douches on the tympanum, and then to gavage as required the facial nerve or the membrane of the tympanum. In its pathological relation, this exaltation of hearing, dependent or independent of hemiplegia, appears to confirm the inductions of Dr. Longet on the intermediate nerve, which ought to be considered as a motor tympanic nerve, being to hearing what the ocular motor nerve is to sight.

19. *On the Constitutional Origin of Erysipelas, and its Treatment.*—Dr. A. J. WALSH has furnished the *Dublin Quarterly Journ.* (Aug. 1850) some remarks on this subject, with cases, which are worthy of consideration.

The following is a summary of his remarks:—

1st. That erysipelas is a constitutional disease, depending solely on a morbid state of the blood; and that the eruption and fever are the means that nature takes to get rid of this poison.

2d. That, for all practical purposes, it is only necessary to divide the disease into idiopathic and traumatic.

3d. That tartar emetic seems to act specifically in erysipelas, by assisting nature in her efforts to throw off the disease.

4th. The best method of administering this medicine is by dissolving one grain in a quart of any bland fluid; the solution to be taken in the twenty-four hours.

5th. That as soon as the tartar emetic has acted sufficiently, sulphate of quina, or some other tonic, is to be administered.

6th. That, if the patient is debilitated, we must administer tonics at the same time that we give the tartar emetic.

7th. That under this treatment the erysipelatous inflammation may spread, but not with the same violence, nor to the same extent, as if the disease were left to itself.

8th. That we shall often require to give aperient medicine during the course of the case, as it is absolutely necessary to keep the bowels free.

9th. That local applications are unnecessary, and often injurious.

10th. That incisions are not necessary, except in the third, or suppurative stage: and if the antimonial treatment be early resorted to, it very rarely occurs that suppuration takes place.

20. *On the Treatment of Pruritis of the Genital, Anal, and Axillary Regions.* By Dr. TOURNIE.—What is generally designated by the term pruritis of the genital and axillary regions, is a cutaneous affection, under the form of prurigo, lichen, or eczema, of which the itching is nothing more than a symptom, and one of the principal phenomena, although it may sometimes exist without any apparent alteration of the skin, and without any other characters than that of the itch itself.

Without constituting a true disease, the itching is not the less a most inconvenient and disagreeable affection; moreover, it may sometimes determine, in nervous persons, consecutive symptoms of manifest importance. We know that onanism and nymphomania often owe their origin to these violent itchings.

There are few practitioners who have not had occasion to observe some one of these troublesome affections; and who, perhaps, have more than once deplored the inability of the materia medica to cure this intractable malady. Among the remedies in common use are corrosive sublimate, borax, sulphur, camphor, iodine, &c. With the aid of these means cures have been obtained; but none, in Dr. Tournié's experience, produced such constant and successful results as the following, which he employed in nine cases of pruritis of which he has reported the details: 1. An ointment of calomel, in the proportion of from 4 to 6 parts of calomel to 30 of axunge. 2. A powder, composed of four-fifths of starch and one-fifth of camphor, well pulverized and mixed. We may, however, in the ointment, increase the proportion of protochloride of mercury; and, in the powder, the dose of camphor, according to the obstinacy of the disease.

The mode of application is as follows: If the diseased parts are covered with scales or dry crusts, as in eczema, then separation is promoted by cataplasms and emollient baths; having accomplished which, he applies, twice a-day, frictions with the foregoing ointment, and, after the frictions, sprinkles the parts with the mixture of starch and camphor.

The ointment alone is inefficacious; and the camphorated starch, without the assistance of the ointment, allays the itch, but does not affect a cure. Experience has proved this.

The following are the cases of which he has reported the details: 1. Prurigo of the labia majora and vulva—intertrigo. 2. Lichenoid affection of the labia majora—intertrigo. 3. Chronic eczema of the scrotum. 4. Lichen in axillary region. 5. Itching of anus. 6. Ditto, do. Besides these, he notices three other cases of perfect cure by this treatment, two of lichenoid affections of the anus in men, and one of prurigo of the labia majora in a woman. He does not give the details of these last three cases. In one only—viz. that of the fourth case—was it necessary to increase, in a notable manner, the doses of calomel and camphor; in all the other cases, the ordinary formula was sufficient to produce a cure.

When the itching of the anus is caused by the presence of ascarides, the calomel and camphor are less efficacious; but he has recourse in this case to mercurial ointment, and often verified its vermicide effects on this species of worm.

The state of pregnancy seems to neutralize the action of the therapeutic means, the success of which he has recorded above. Thus the itching of the genitals in two pregnant females proved intractable to every kind of treatment. In one only venesection seemed to allay the itchiness a little; but in the second it produced no effect;—in both, the itching disappeared immediately after delivery. In these two cases the itching had commenced during pregnancy; and it existed without giving the vulva any appearance of a lichenoid or pruriginous eruption.

Dr. Maslieurat-Lagémard has published, in the 12th No. of the "*Gazette Médicale*," for 1848, page 204, a very curious and remarkable account of a general itching supervening, without eruption, in the course of eight pregnancies, in one of his patients. This did not commence till after the sixth month, and no treatment appeared to alleviate the violent itchings, which, during the eight pregnancies, resulted six times in premature deliveries.

In Dr. T.'s hands, the combination of the calomel ointment with the powder

of camphorated starch, in the treatment of itching of the hemorrhoidal region, has given results which no other treatment has ever obtained in so constant a manner.

Every practitioner who has been consulted for these affections has been obliged to direct himself to formulae of great variety, before hitting on a successful expedient. In illustration of this, it is sufficient to relate the summary of five observations published by Dr. Ruan, in "Hufeland's Journal."

Of these five cases, two were treated without any success by purgatives, diuretics, opium in large doses, Goulard's extract, and solution of borate of soda; intractable to all these means, they were cured by the balsam of copaiva.

In a third case, the copaiva, which had succeeded in the two first, was inefficacious; whilst the borax, which had failed in the others, caused the itching to disappear in this case.

A fourth case resisted the divers means employed for the first three and gave way to the administration of the subcarbonate of soda in small and repeated doses.

The fifth case resisted every treatment.

[Dr. Tournié takes no notice of hydrocyanic acid, which, in the proportion of a drachm to eight ounces of almond emulsion, we have found frequently to give great relief. In *prurigo ani* and *puendi*, we have more frequently succeeded in effecting a cure with a weak solution of corrosive sublimate (one grain to eight ounces) than with any other application. The state of the digestive function and urinary secretion should always be attended to, whatever be the local treatment.]—*Monthly Journ. Med. Sci.*, April, 1851, from *L'Union Médicale*, 16th Jan.

21. *Creasote in Diarrhœa*.—Mr. KESTIVEN extols (*London Med. Gaz.*, Feb. 1851) the efficacy of creasote in the cure of diarrhœa. The form in which he used it was: R.—Creasoti ℥j to ℥v; Spt. ammon. arom. ℥xv to ʒj; Aq. ʒj to ʒiss. Where pain has been severe, Tinct. camph. co. has been added.

In no single case, Mr. K. says, has creasote failed to be of signal benefit; in most cases one single dose was sufficient to arrest the course of the disease; in very few instances has it been requisite to administer more than the second dose.

22. *Polypus of the Larynx*.—In our preceding No. will be found an elaborate review of the excellent work of Dr. Ehrmann on this affection. The following case, recently recorded by Dr. PAGLI (*Medicinisches Correspondenz-Blatt*), is interesting in connection with this subject.

A tailor, 36 years of age, affected for two years with a dry cough, accompanied with very marked emaciation, and complaining continually of a foreign body in his throat, was supposed to be affected with phthisis. One morning, in a violent paroxysm of coughing, he threw up a vascular polypus, the length of half a finger, and from this moment he slowly began to recover.—*Gaz. Méd. de Paris*, Jan. 4, 1851.

## SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

23. *Diffused Femoral Aneurism—Ligature of the Internal Iliac Artery—Recovery*.—Mr. G. SAUNDERS, Assist. Surg. 47th Reg., has recorded, in the *Medical Times* (March 29th, 1851), the case of a soldier, nineteen years of age, admitted into hospital Aug. 8th, 1849, for a painful swelling in the left groin, which rather suddenly appeared five days previously, when at light infantry drill. The swelling was diffused and extended beneath Poupert's ligament; rather hard, immovable, having a central pointing of a dark blue appearance, and much surrounding inflammation of the integuments, but no pulsation; leeches and

cold lotions were applied to the part; purgatives, antimonials, confinement to bed, and spoon diet ordered. By these means the inflammatory state of the parts was much reduced. The next day the swelling was more pointed, with indistinct fluctuation. In a few days the integuments became thin and dark in colour; a small slough separated, and there was considerable oozing of arterial blood; pulsation in the tumour was now very audible through the stethoscope. The bleeding was arrested by pressure.

On the 21st the swelling became more diffused; base hard; much hemorrhage; great pain on pressure above Poupart's ligament; no pulsation in the artery immediately below the swelling, or in the corresponding popliteal space, with a diminution of the natural temperature of the limb.

On the following morning Mr. Battersby tied the external iliac artery of the left side about three-fourths of an inch above Poupart's ligament. The integuments were in the first place divided, to nearly four inches in extent, along the upper margin of Poupart's ligament, various layers having been cut through and much cellular tissue displaced, and some fat removed, directly beneath which the artery was exposed. The ligature was passed from the inner side of the artery without the least difficulty, and the vessel properly secured. Immediately this was done, pulsation in the tumour ceased; the concentric coagula forming the aneurismal sac subsequently sloughed away; the ligature separated on the twenty-first day after the operation, and on the 18th of October the man was discharged quite well.

**24. Femoral Aneurism.—Cure in twenty-four hours.**—Staff-Surgeon G. R. DARNELL records, in the *Medical Times* (Feb. 8th, 1851), a very interesting case of femoral aneurism in a soldier, aged 35, who had always enjoyed good health. The patient was admitted into the Regimental Hospital, Chatham, on the 16th May, complaining of pain in the lumbar region and left side below the ribs, occasioned, as he thought, by exposure to cold. On the 18th, the pain of back and side was entirely removed, but he directed attention to a pulsating tumour, about the size of a pullet's egg, on the anterior aspect of the left thigh, occupying a portion of the triangular space, within two inches of Poupart's ligament. He stated that he had only observed the swelling that morning, and that he was not aware of having made any violent exertion lately; but that for some time previously he had experienced a sensation of numbness and slight pain in the leg and instep.

The tumour in the groin is now rather larger than the longitudinal section of a hen's egg. It is pulsating very powerfully with a distinct bruit; is firm and elastic to the feel, but can be partially emptied of its contents by the pressure of the hand, and the pulsation entirely ceases under firm compression of the inguinal artery by the fingers. The circumference of the diseased thigh, over the tumour, is  $19\frac{1}{2}$  inches, that of the sound one 19 inches. He has some numbness in the leg, extending from the knee to the foot. Temperature of the limb natural; pulse at the wrist 80. Considerable diffuse pulsation is observable over the abdominal aorta, a little below and to the right of the scrobiculus cordis; the stethoscope gives, at this point, a very distinct bruit; a faint rasping sound at the site of the mitral valve, and a double bellows-murmur at the apex of the heart; general health good; bowels regular. Rest in the recumbent posture; low diet.

31st, 10 A. M.—No change.

3 P. M.—Aneurismal compressor applied, the pad of the instrument being placed over the external iliac artery, as it passes over the pubis, two inches above the upper edge of the tumour. The artery is under the complete control of the instrument, but its power is so regulated that the current of blood is only partially checked, a feeble pulsation being still perceptible in the tumour.

6 P. M.—Pressure of the pad rather irksome; still slight pulsation in the tumour; temperature of the limb reduced, with a numbness about the knee. Pad slightly shifted; pressure increased by a few turns of the screws.

9 P. M.—No pulsation in the swelling; pain from pressure of the pad a good deal complained of; but patient having been made fully aware of the nature of his complaint, and the principle of the treatment, bears the pain without the



least fretting or restlessness. Numbness and coldness of the limb increased; pressure slackened by a few backward turns of the screws, and the blood permitted again to pass feebly through the aneurismal sac. Frictions to the limb, and bottles of hot water to the foot. Some thirst; no arterial excitement; pulse 80.

June 1, 6 A. M.—Has had no sleep, and still complains of the pressure of the instrument. Tumour still pulsating feebly, but begins to have a firmer feel; numbness rather less, and temperature of the limb returning; no swelling or discoloration from obstruction of the venous circulation. Pad lifted from its position, the artery being commanded by the fingers a little higher up; integuments under the part very slightly reddened; parts cooled and sponged with cold water; instrument re-applied in quarter of an hour.

10 A. M.—Temperature of the limb nearly natural; very little uneasiness from the compressor since the morning visit; patient tranquil and in good spirits.

1 P. M.—Feeble pulsation still perceptible, but the tumour loses more of its elastic feel, and it is evident that the formation of fibrine is going on; limb of natural warmth; numbness less; pad sewed down so as to stop the pulsation completely.

3 P. M.—No pulsation since last report; the aneurismal swelling has entirely lost its elasticity, and appears to be completely solidified; very little uneasiness from the instrument, which is left undisturbed. Twenty-four hours since its first application.

8 P. M.—Pressure entirely removed by lifting the spring; tumour still without the slightest pulsation, and as solid as a stone; compressor re-applied after a few minutes, but with diminished power.

June 2, 6 A. M.—Patient has had some sleep, and has no discomfort or inconvenience from the truss; tumour solid, contracted, and pulseless; temperature of the whole limb natural; sense of numbness greatly diminished; complains of a little pain in the fore part of the leg, and slight thirst.

8 P. M.—Remains as at the morning visit nearly, but feels more comfortable; continue the instrument as a security.

June 3, 10 A. M., 67 hours.—Has had a good night's sleep; tumour further reduced in size; temperature natural; posterior tibial artery felt beating feebly. The truss removed altogether; integuments under the pad scarcely reddened; patient quite comfortable, and general circulation tranquil; pulse 75; tongue clean; no thirst, bowels confined. R.—Ol. ricini  $\mathfrak{z}\mathfrak{j}$ . Spongo the limb with cold water.

8 P. M.—Bowels once moved by the oil; patient continues well and free from all complaint; tumour solid, and somewhat flattened in shape.

June 4, 10 A. M.—Patient slept well; has a little pain in the leg and instep, and is weak from confinement to bed and reduced diet; in other respects quite well. The instrument has not been re-applied.

Oct. 25th.—Nothing further to detail from the date of last report; the absorption of the tumour has been slow but progressive, and scarcely a vestige of it now remains. The patient is in excellent health, and has apparently quite lost the threatening symptoms of general arterial disease he had on his admission into hospital; he still, however, complains of a very slight sense of numbness about the inside of the knee, and a trifling degree of weakness of the limb; but these symptoms are gradually wearing away. He was discharged on the 26th Sept. for two months' sick furlough, previous to rejoining his regiment, it being thought inexpedient to subject him too soon to the very severe and fatiguing duties of drill.

At the risk of being thought tedious, I have given the notes of the foregoing case in detail, conceiving that it may be thought interesting to trace the progress of the cure. The visits, it will be observed, were made at short intervals; and I am of opinion that, in the treatment of aneurism by compression, it is of the greatest importance that the surgeon who undertakes the care of the case should be unremitting in his attentions to the patient. Much of the success depends upon his watchfulness, not only in regulating with nicety the stream of blood

through the sac, and in watching for the favourable moment when it arrives to stop it altogether; but in alleviating, as far as possible, the sufferings of the patient from the continued pressure of the pad, by such delicate and careful manipulations, or shiftings of the instrument, as it is practicable to make. This is especially requisite in cases where a second or relieving clamp cannot be applied.

The critical moment, it appears to me, when the current of blood may be stopped altogether with effect, is when the deposit of fibrine has begun to take place, as evidenced by the loss of elasticity, and increasing firmness of the tumour: and when the collateral circulation has become fairly established, as indicated by the restoration of the natural temperature of the limb, though the stream may perhaps very judiciously be suspended now and then, for a short interval of two or three hours at an earlier period, and then be permitted to flow again as before. These occasional checks throw the impetus of the arterial current with new and greater force upon the anastomosing branches, thus tending to their enlargement.

The result of the above case is interesting, from the rapidity of the cure, as well as its apparent completeness under what seemed to be adverse and discouraging circumstances: and it is satisfactory to remark that no local injury whatever was sustained from the instrument, although the pressure was continuous, there having been no means of applying a second elsewhere in the line of the artery, as recommended by Mr. Bellingham, and practised with so much relief in cases of popliteal aneurism. The integuments under the pad were hardly reddened, and the sufferings of the patient after the first few hours were comparatively trifling.

The instrument used in this case, as a *Presse Artère*, was a very simple modification of an improved hernial truss invented by myself. The truss selected was a double one, with a strong spring, the extremity of which, on the left side, was slightly altered in its curve, so as to bring the pad directly over the external iliac artery, as it emerged from beneath Poupert's ligament. For the ordinary wooden pad of this truss were substituted two oval plates of steel, the upper one attached firmly to the end of the spring; the lower connected with the first by four long screws, so managed as to increase or diminish the space between the plates, thus enabling the surgeon to gain a small amount of additional power. To the lower plate was fixed a rounded piece of cork, covered with a cap of chamois leather, and modelled with a gentle ridge or elevation along the centre, which was invented to prevent undue pressure on the vein or nerve on either side, while the ridge itself pressed directly upon the artery. The perineal strap of a common truss was attached, which, when buttoned to the upper plate, augmented the resisting power of the spring, and, consequently, the additional force of the screws, which would otherwise be divided equally between the spring and the part compressed.

I am far from supposing the instrument above described to be perfect, but, in the absence of a more legitimate one, it was the readiest contrivance I could command, and it answered the purpose in this case extremely well. I have since learned that a somewhat similar apparatus was made use of, several years ago, by the late Mr. Todd, of Dublin, in his attempt—the first, I believe, that was ever made—at the cure of a case of femoral aneurism by compression. An account of that very interesting case, and of the instrument, will be found in the third volume of the Dublin Hospital Reports.

25. *Ligature of the External Iliac Artery*.—Dr. CHARLES HALPIN communicated to the Surgical Society of Ireland a case in which he applied a ligature to the external iliac artery to arrest hemorrhage from the femoral, produced by the latter artery being opened by the ulceration of a bubo in the right groin. The subject of it was a soldier aged 21. Hemorrhage occurred on the 11th Sept., and again on the 12th, and on the afternoon of the latter the ligature was applied; on the 14th, 15th, 16th, and 19th Sept., venous hemorrhage occurred from the site of the bubo; and on the 20th, arterial hemorrhage; and on the evening of the last-named day, the patient expired.

A post-mortem examination was made on the 21st day of September, nineteen hours after death. Assisted by Dr. Ros and Mr. Brice, a most minute examination was made of the parts connected with the disease and the site of the operation. The peritonæum, which had not been wounded in the operation, was found adherent to the cæcum; it was easily torn in separating it from this part. The artery was exposed from the bifurcation of the aorta to the middle of the thigh. The ligature applied on the 12th instant to the external iliac was about two inches above Poupart's ligament, and was very nearly cast off, being retained by some portion of the external coat only; the artery had not been disturbed in the operation to a greater extent than permitted the needle to pass under and between it and the vein.

It was evident that the hemorrhage immediately preceding death had come from this portion of the artery, notwithstanding its being filled from the ligature to the giving off the internal iliac artery with a very firm coagulum.

About two inches below Poupart's ligament, there was an *aneurism* of the femoral artery, capable of containing a small chestnut. The origin of the profunda femoris was implicated in this aneurism.

The femoral vein was destroyed for about three inches of its course, below Poupart's ligament.

The ulceration connected with the bubo, reaching the aneurismal sac, eroding its coats, originated the hemorrhage on the 10th instant, which terminated in the death of the patient on the 20th; but I am strongly of opinion, at the same time, that the blood which came away on the 14th, 15th, and 16th instants, and which had all the character of venous blood, descended through the iliac vein, and escaped by the femoral vein, which had been destroyed by ulceration. I am not aware how far such a source of hemorrhage has been observed or suspected before, but it strikes me that the possibility of such an occurrence is worthy of the attention of the profession.—*Dublin Medical Press*, May 14th, 1851.

26. *Hernia—Operation without opening the Sac.*—The subject of hernia is a most important one to the practical surgeon; and there is no class of cases which requires so much study and investigation. The affection is one which is so common, and so likely to be brought under his notice at any period, in its most difficult and dangerous aspects, that he is bound to make himself acquainted with its nature and treatment, and with the various circumstances which so frequently obtain, to modify both. It at first sight appears somewhat surprising that there should be so much difficulty connected with an apparently simple subject, and that there should have arisen so much controversy upon the most effectual modes of combating the dangers which are induced when a patient is labouring under a strangulated hernia: for upon no one subject, perhaps, has so much been written as upon this. The writings of Cooper, Pott, and Lawrence, with many others, bear testimony to the laborious investigation which these eminent men have been engaged in to ascertain the precise nature of the affection, and to simplify the treatment of it both by operation and other means. This surprise, however, ceases when we come to see the disease at all on a large scale; for then it is we find that there are no two cases exactly alike; and that, in one case after another, circumstances obtain or arise which the surgeon has not noticed before, or has altogether forgotten; and he accordingly finds that he is compelled to modify his mode of operation and his subsequent plan of treatment, according as the singularity or gravity of these circumstances is met with. Thus it is that he who wishes to make himself well acquainted with the pathology of hernia, and with all the circumstances which are constantly interfering to modify its treatment, should not miss a single opportunity which may present itself of observing every case which may fall in his way. It may be stated as a great probability that, with every fresh case, something new will be learned. Hence we consider a faithful and accurate report of all individual cases of hernia to be most especially useful. More of course will be learned by the actual ocular observation of such cases; nevertheless, an attentive observer will add to his store of knowledge by reading the records of such when he cannot see them for himself.

The point which has been of late, and is now mostly under discussion, is as to the propriety and expediency of performing the operation for strangulated hernia without laying open the sac: other matters connected with the treatment of the affection—such as operating at an early period after strangulation, not using too prolonged taxis, and refraining from the employment of powerful aperients too early after the stricture upon the gut has been relieved—have been satisfactorily settled in the minds of all judicious surgeons; but the one point under consideration is that just mentioned, and it is likely to agitate the minds of practitioners for some time yet; at all events, until a larger mass of well-recorded cases of hernia treated by the two separate methods of operating with and without opening the peritoneal cavity, shall be brought home to the profession. It must be confessed by all those who have paid any particular attention to this very important subject, that the data are not yet sufficient to justify the surgeon in coming to anything like a correct conclusion as to which is the safest and best mode of operating generally; nevertheless, the researches of Key, Luke, Gay, and Hancock have contributed much to enlighten the profession on this matter, and the minds of surgeons are thus imbued with the necessity of considering every case of hernia with extreme care before they determine upon the manner in which such and such a case should be treated. The feeling is gaining ground that the operation without opening the sac is applicable to very many cases of strangulated hernia, where a few years ago the peritonium would have been opened without the least consideration, and supposedly as a matter of necessity; that also the danger of the proceeding is much lessened if so delicate and easily inflamed a structure as the peritonium is not meddled with. Experience has already amply shown the truth of this; but, on the other hand, it cannot be denied that there are many good surgeons who look upon this mode of operating not only with great suspicion, but who consider it to be both unsatisfactory and more dangerous than when the sac is opened, its contents brought into view, and the condition of the parts well ascertained before they are returned into the abdomen. Those who are advocates of the former method urge as arguments in its favour the great danger of peritonitis where the membrane is cut into and the intestine itself is rudely handled—in fact, the circumstances that in almost every case which is fatal after the ordinary operation, a greater or less amount of inflammation of the peritonium and its consequences is found; and hence the immunity which obtains when a simple incision is made through the overlying textures. Those who oppose this plan urge—and justly too—that it is not applicable to a great number of cases, and that there must always be an extreme difficulty in selecting those to which it really is applicable: moreover, that the peritonium is not so liable to be inflamed by a simple incision into it as is imagined by some, and that it is impossible to tell at the time of operation whether or not the stricture be fairly relieved, and whether the intestine itself be in a fit state or not to be returned into the belly. Such, then, is the position which this question at present assumes, and is likely to maintain, until the experience of a large number of cases has been fully brought before the profession. It therefore, we conceive, becomes the duty of every one who has the opportunity of bringing forward facts which may throw light upon this subject, to do so; and we shall make it a point of recording every case of hernia in the reports which it is our duty from time to time to make from among those which are treated in this hospital.

Before proceeding to relate two instances of strangulated hernia where the operation has been successfully performed without opening the sac, we will allude to the report of a case which we inserted from this hospital a few weeks ago. In this instance it will be remembered that the patient was an old man, in whom strangulation of the intestine had existed for less than 24 hours, and everything appeared to be favourable for the operation without opening the sac; and yet, on post-mortem examination, a considerable portion of intestine was found to be in a condition quite unfit to be returned, although the stricture had been effectually relieved by cutting it outside the peritonium. We at that time made the remark that this very case illustrated the difficulty which must necessarily exist in arriving at a correct conclusion as to the state of the parts inside, the hernial sac, and was *per se* an instance to show the danger and inexpediency

of returning a portion of intestine which has been constricted, even for a short period, without first ascertaining its condition. We have now, on the contrary, to relate two instances where the same kind of operation has admirably succeeded, and which thereby tell us arguments in favour of operating without opening the sac, if possible.

Mary Ross, aged 48, married, was admitted into King's College Hospital with a strangulated femoral hernia, on Tuesday, November 26th. She stated that on Monday evening, at 8 P. M., whilst sitting before the fire, she was suddenly seized with a severe pain in the lower part of her belly. She at once sent for a medical man, who gave her an emetic which acted violently, and increased the pain under which she was suffering. About 11 o'clock of the morning she was admitted when she first herself noticed a tumour in the right groin, which was very tender to the touch. She had never observed a swelling there before. At 2 P. M. she was sent into the hospital, *no attempt whatever having been made to return the hernia before admission.* Mr. Fergusson pronounced the tumour to be a femoral hernia: the patient was at once placed under the influence of chloroform, and a careful attempt was made to return the swelling by the taxis; but Mr. Fergusson, finding that it did not yield in any way, determined to operate without further delay. Making a very short incision over the hernial tumour, he dissected through the separate tissues, and carried the knife inwards, and divided Gimbernat's ligament, outside the sac: the contents were at once returned into the belly; little blood was lost; the edges of the wound were united by sutures; a compress and bandage were applied.

27th.—This patient suffers no pain; has slept well the preceding night, but the bowels have not been opened since this operation.

28th.—Bowels have been opened by means of an enema this morning. The patient feels comfortable.

Dec. 3d.—Stitches have been taken out of the wound, and this is dressed with adhesive straps.

Discharged on the 7th.

Lydia Boothby, æt. forty-six, married, and mother of eleven children, has had a crural rupture on the right side for the last six years, and has worn a truss for the last twelve months. On the night of December 23d, at ten, she was seized with vomiting and a dragging pain in the stomach, whereupon she attempted to return the tumour with the help of her husband, but found she was unable to do so. These efforts at reduction were continued all night; and, as they were unsuccessful, she applied and was admitted into the hospital. Next day, Dec. 24, she had vomited fecal matter before she came in. She had no vomiting when admitted, but complained of a severe dragging pain at the stomach, and shooting pain over the abdomen. There was a tumour, the size of an egg, in the left groin, turning up over Ponpart's ligament: it was hard, and felt nodulated at the upper part, but was smooth and fluctuated at the lower. Mr. Fergusson saw the patient, and, finding that he was unable to return the hernia, proceeded to the operation. He first made a very limited incision just over the neck of the tumour, and divided the separate tissues over the sac; he then passed the knife upwards and inwards, and cut Gimbernat's ligament, and was then enabled to pass the contents of the sac into the belly without opening the sac. It was necessary, however, after dividing the ligament, to cut some fibres between that tissue and the sac; the rupture after this was returned with the greatest facility.

On the same evening this patient found herself much relieved; there was no pain, no tenderness nor sickness.

On the next day her bowels were freely opened, and the only thing she complained of was a little fever, which required some saline medicine. The wound soon put on a healthy appearance, and she rapidly recovered.

Both these cases are interesting, inasmuch as the same operation was put in force in either, and that, too, with the greatest success, although the two cases were not by any means alike. The comparatively simple and harmless operation without opening the sac was performed, and both patients recovered without a single bad symptom. Both cases illustrate the fact that there are certain instances of strangulated hernia to which this peculiar operation is decidedly

applicable; and they teach us what are the circumstances which will lead the surgeon to judge that he is dealing with a fit case for such a mode of procedure. In the first patient, the conditions were just those which are considered to be the most favourable for attempting the operation without cutting into the peritoneum. The woman had never suffered from hernia: in fact, it was quite recent descent of bowel; the parts had not been suffered to remain long down, and, moreover, it was a femoral hernia, which is probably that kind of hernia in which the operation of not opening the sac is most likely to succeed frequently. The anticipations of the surgeon that in such a case the operation might be completed without difficulty were verified. In the second case, the same conditions did not obtain; for here the hernia was not recent, but was of six years' standing, and for twelve months the patient had worn a truss; and it might with propriety be expected that some changes would have been brought about in the sac which would oppose a successful attempt at reducing the hernia without an exposure of the cavity. Nevertheless, there was an equal amount of facility in either case in completing the operation. There was also this difference in the two cases—in the one it has been seen that no attempt whatever had been made to return the hernia, either by the patient herself, or by a medical practitioner; in the second instance, both forcible and prolonged attempts were made by the patient and her husband to push the tumour back. The circumstances in each were therefore different, and certainly much more unfavourable in the latter case. Still this rude handling of the parts had not appeared to produce any mischief. A great point in the success of both cases consisted, doubtlessly, in early recourse to operation—a measure which should always be adopted whenever it is considered that the employment of the knife is necessary. It must not, however, be considered that in all cases like these just mentioned, and more especially the first operation of not opening the sac, will invariably succeed or can be accomplished; nor is it to be supposed that the intestines must necessarily be in a fit condition to be returned because strangulation has existed only for a few hours, and because those symptoms which not unfrequently indicate a gangrenous condition of the intestines are not at all perceptible. The case of the man first alluded to shows the impossibility of deciding this question; for although in this instance strangulation had not existed for twenty-four hours, death rapidly ensued after operation, and the intestines was found in a very disorganized condition; and it is often quite impossible to judge, from the absence of symptoms, as to the comparatively healthy state of the contents of a hernial sac. A few days ago we were present at an operation in a remarkable case of strangulated femoral hernia in a very aged person, where there were not the slightest symptoms to indicate that the intestine was in anything but a healthy condition. The operation without opening the sac was performed. The stricture was fully relieved, but the patient died; and on post-mortem examination a considerable portion of intestine was found to be in a semi-gangrenous condition. This uncertainty as to the state of the intestine must then be looked upon as one of the great impediments to, and difficulties in connection with, the adoption of this particular plan of operating. Still it must be hoped that a careful observance of signs and symptoms, and the experience of a greater number of cases than have hitherto been brought forward, may be able to enlighten the surgeon on this serious point, and enable him to avoid the dangers which must necessarily arise if he adopts too hastily or too generally either plan of operating.

Mr. Fergusson has lately adopted a mode of operating by a very small external incision through the skin, just over the neck of the tumour. It was formerly his custom to recommend and practice much freer incisions, of various shapes, over the surface of the tumour; but he now proceeds in a manner somewhat like that recommended by Mr. Gay in his very beautiful work on Femoral Hernia: by means of this incision he is enabled to get easily at the Gimbernat's ligament, which it is generally necessary to divide. Should, however, a larger incision be found necessary, it is very easy to extend it over the tumour. Mr. Gay practices his incision, which is even more limited than that of Mr. Fergusson, by the side of the neck of the hernia, and does not interfere with the tumour at all if he can possibly effect his object without it. Mr. Fergusson, in both the

cases narrated, made a small cut just over the neck of the hernia, but interfered little with the main portion of it. The advantage is that so small a wound very soon heals up.—*London Med. Gaz.*, Jan. 1851.

27. *Puncture Wound of the Abdomen—Recovery.*—Dr. H. C. FIELD communicated to the Surgical Society of Ireland the following remarkable case of puncture wound of the abdomen, accompanied with an opening into the intestinal canal, and great hemorrhage, in which the patient recovered:—

On the 22d of a severe November evening, I was requested, in a very hurried manner, to visit a gentleman who, I was told, had, while sitting at dinner, some disagreement with a member of his family, and, in fit of irritation, exclaimed that he was tired of life, and had immediately inflicted some serious injury on himself. On entering his room, I perceived that it bore a most appalling appearance: the floor was covered with blood, and my patient was extended on a sofa in a remote corner of it, apparently lifeless. I could not feel any pulse. I immediately called for some brandy, which I obliged him, with great difficulty, to swallow some of. I soon perceived that blood was flowing in considerable quantity from a wound in his abdomen, which was situated to the left side of, and near to, the umbilicus. I soon learned that the wound had been inflicted by a large *carving knife*, which, from the stained appearance it presented, and from the account I received, as well as from the nature of the wound, I judged had passed into the cavity to the distance of *five inches*, in a direction obliquely backwards, downwards, and to the left side. The blood, which was of a dark colour, flowed freely in one continued stream, and hemorrhage was so excessive as to excite an apprehension of immediate dissolution. However, having failed to suppress the hemorrhage by firm compress and bandage, and convinced that a few minutes more of such bleeding must end in death, I introduced to the bottom of the wound a *dossil of lint*, the end being kept outside, and applied over it a graduated compress and a broad bandage firmly. This succeeded in arresting the further flow of blood, and a cordial draught, with forty drops of laudanum, was then administered. A feeble tremulous pulse at the wrist could now be felt, but he appeared in a sort of faint. He was bathed in a cold ewent, his extremities lay still and motionless, he vomited at intervals, and his urine and bloody feces passed unconscious to him, and his respiration was slow and laboured. In this state, almost bordering on death, he remained for seven hours. About this time the vital power first showed a disposition to reaction. He expressed, in a few broken accents, the general uneasiness felt, and complained of pain and numbness in the lower extremities, particularly in that of the left side: this, together with a peculiar restlessness, disturbed him during the remainder of the night, which was passed without any sleep, notwithstanding the anodyne he had taken.

23d. A medical attendant remained with him during the night; there was no recurrence of hemorrhage; he still appeared in a state of extreme debility, and felt great nausea, which occasionally increased to hiccough and vomiting; he also complained of numbness and pain extending down the left limb to the foot; there was slight tenderness about the wound on pressure; pulse 120, feeble, and irregular. With a view to relieve the state of the stomach, as well as to allay the general irritability, and ensure perfect rest, I directed the saline effervescing mixture every four hours, with fifteen drops of laudanum; also fomentations to the abdomen. Evening: He still complained of pain and numbness in the thigh and leg; the tenderness of the abdomen had increased; there was thirst; pulse 110 and feeble; tongue white and furred; skin hot and dry; stomach relieved. Ordered to continue the draughts at longer intervals, also the use of ice, and the fomentations every four hours.

24th. He had some quiet and refreshing sleep during the night, and felt himself relieved; his pulse was 90; skin cool; no sickness of stomach; he had two stools, each containing a large quantity of dark clotted blood; pain and numbness in the left limb not so troublesome; tenderness of the abdomen on pressure continued. Ordered the fomentations to be repeated.

25th. He had several evacuations, each containing a quantity of clotted blood;

fever much less; pulse 90; tongue foul; pain and numbness in the extremity still distressed him; there was little or no pain about the wound; the dressings came away, when a quantity of *feculent* matter, mixed with blood and some pus, was discharged.

26th. Has had some refreshing sleep, and was much better this morning.

27th. Continued to improve; the evacuations were free from blood; he now complained of occasional starting in the left limb, in addition to the pain and numbness, which still continued, though in a less degree; the wound discharged healthy pus unmixed with feculent matter; he was free from fever.

I think it unnecessary to delay the Society by detailing the daily reports: he continued to improve daily: the wound gradually healed. The peculiar sensations of numbness, pain, starting, and tingling in the left leg and thigh, however, continued, and were occasionally very distressing, although on the whole less severe and frequent in their attacks.

On the 8th of December he attempted to stand, but the left leg was so weak as to yield beneath his weight.

January 2d. Since the last report, his health and strength considerably improved; for some time the left extremity was nearly paralytic, though frequently attacked with paroxysms of pain and numbness, which latter he described as more troublesome and distressing than the former. These paroxysms, however, were so slight and so few that every hope was entertained of his perfect recovery at no very distant period.

I cannot close this statement without directing the attention of the Society particularly to some of the circumstances of this case, as I conceive they are calculated to excite our astonishment, as well as our admiration, at the extraordinary resources of nature, whereby she is enabled, under the most unfortunate circumstances, to preserve the life of a human being. In this case we have evidence not only of the peritoneum, but also of the intestinal canal, being opened.

I should presume, from the feculent quality of the discharge, that some part of the colon was injured. What blood-vessel was wounded cannot with certainty be affirmed, but most probably some large branch of the vena portæ, such as the inferior mesenteric, or one of the colic veins. I conceive there can be no doubt, from the peculiar sensations complained of in the lower extremities, that some branches of the lumbar plexus of nerves must have been divided, or otherwise injured, at the time of the accident. It appears a strange anomaly in the history of wounds of the abdomen, that so little peritoneal inflammation was excited in this case, notwithstanding the effusion of blood and feculent matter. May we not in some degree account for the fortunate absence of this, so common a result in general, either by the copious hemorrhage which immediately followed, reducing the system to so low a condition as to retard the reaction in the circulating organs, and so oppose the attack of local inflammation or general fever, or a coagulum of blood may have so formed around the wounded vessel as that further bleeding and effusion into the cavity were restrained until the adhesive inflammation set in, by means of which a permanent recovery was effected in a surprisingly short time, for we find him in less than a fortnight free from every unpleasant symptom, excepting the pain and numbness in the lower extremity, which may be referred to the injury of some of the nervous filaments of the limb? I have always entertained the belief that we are never too old to learn. I know, although the case recovered, that there was unavoidable error in some of the practice, and I hope to hear some remarks upon it from some of the experienced surgeons I have the honour to see around me.—*Dublin Medical Press*, April 16, 1851.

28. *Tracheotomy in a Child thirteen months old successfully performed for Œdema of Glottis.*—T. L. WHISTLER communicated to the Surgical Society of Ireland (Feb. 8) an interesting case of œdema of glottis in a child only thirteen months old, caused by the taking of boiling water into the mouth, in which antiphlogistic measures having failed to afford relief, tracheotomy was successfully performed.

The subject of it was an unusually fine boy, brought to Mr. W. at eleven



o'clock on the morning of the 13th Jan., having, as stated by his mother, swallowed boiling water from a kettle removed to the side of the fire-place, on a level with the child when standing, at a moment when left alone. It was crying violently, and evidently suffering much pain; the face was greatly flushed, and the pulse much accelerated; there was, however, no vesication about the mouth, and but slight tumefaction observable on the fauces. I directed four leeches to the throat, and after an emetic (which I subsequently found was not administered), a grain of calomel and quarter of a grain of hippo to be given every second hour. I again saw the child at three o'clock; the leeches had freely bled, and the calomel was punctually dropped on the tongue; the breathing was rapid, but not oppressed; the usual febrile symptoms intense. When I again visited the child at eight o'clock in the evening, every alarming symptom had set in—difficult croupy respiration, pale bloated face, cold skin, pulse scarcely to be felt, with inclination to stupor. The child was dying, and no prospect of saving life was left but to open the trachea—an opinion which was at once concurred in by Dr. Darby, who kindly and effectively gave me his assistance.

After the first incision, the convulsive struggles of the child, the venous hemorrhage, the rapid working of the trachea, and the insufficient light procured in the cabin, rendered the remaining steps of the operation distressing, and more than usually tedious. Not being at the moment supplied with the double hook, I laid hold of the trachea with a common tenaculum, which, being divided, the air forcibly rushed out, followed by a discharge of blood and mucus. From the situation of the child, and anxious to stop the hemorrhage as quickly as possible, the canula was introduced sideways without removing any portion of the trachea. In a few seconds the child breathed freely through the wound, occasionally forcing up large collections of bloody mucus, consciousness returned, and wine and water was shortly afterwards swallowed. The examination of the fauces with the finger conveyed the impression as of a large cherry in the situation of the epiglottis. Directions were left with the mother to remove the mucus from the canula with a feather and by suction, and to continue the powders of mercury and chalk.

On the following morning we found that the child had passed a tolerably quiet night, discharging a profuse quantity of mucus through the tube, and much troubled with cough; the fauces were much swollen; the epiglottis hard and very tumid, the uvula presenting the appearance of hoiled parchment. A blister was applied over the sternum, the mercury and hippo continued, with nourishing diet. It is unnecessary to record each day's report of this case; severe bronchitis threatened on two occasions, but was successfully subdued by active treatment. I may mention that so intense was the injury sustained by the larynx, that six days elapsed from the operation before the tube could be removed, each trial made by placing the finger on the opening being followed by a most distressing cough and dyspnoea. The child became much emaciated, but is now daily improving in appearance and spirits.

I conceived it might not be unimportant to bring forward this case as a record amongst a class of accidents frequently to be met with in hospital and dispensary practice, not from any novelty in the mode of treatment adopted, or the success which, under similar unfavourable circumstances, has attended its performance in the hands of others, but that every additional case to those already published may inspire the junior portion of the profession with confidence in having recourse to the operation before too much valuable time has been allowed to pass over, and thus that an operation of such unquestionable advantage be not brought into disrepute by undertaking it when no fair prospect of success remained. Its difficulty is universally acknowledged to be greater in the child than in the adult, owing to the smaller size of the trachea, its generally increased distance, the hemorrhage, the struggles and convulsive startings of the infant, which must often tend to embarrass the most experienced surgeon. These difficulties are in part removed by seizing the trachea with a tenaculum—a plan first recommended, I believe, by Dr. Murray of Dnlin in 1827, improved on by Mr. Carmichael, who used a double hook, and removed with a scissors a circular portion of the tube, as successfully adopted by him

in the case of a child, aged two years, attacked with inflammation of the glottis from boiling water. By some the removal of a portion of the trachea is not recommended. M. Velpeau deems the "simple incision always sufficient for the introduction of the canula, whilst the removal of a slip of the trachea is likely to be followed after cicatrization by an irremediable diminution of its diameter."

Mr. Porter is of opinion that, if the operation be performed on the larynx, there can be no objection to removing a portion, "because the wound is not extensive, and the hemorrhage must be trivial; but the trachea lies deeper, there is more likelihood of meeting with a troublesome or embarrassing flow of blood to delay the operation, and, even supposing that no vessel of consequence is wounded, the patient at every inspiration sucks in a quantity of blood from the open sides of the incision, and the cough, expectoration of bloody mucus, and other harassing symptoms, are continued."

In the present case, I was influenced by the great necessity there existed to stop all further bleeding, and give at once a supply of air, from the want of which at one time I thought the child had expired.

In conclusion, I wish to impress on the junior portion of the profession present that the failure of tracheotomy in œdema of the glottis, the result of taking boiling water into the mouth, would seem to be in general attributable either to the operation having been inefficiently performed, or delayed until congestion or effusion of head or thorax had set in. The present case was one which held out but faint hopes of ultimate recovery, owing to the age of the infant, the unfavourable season of the year, and the impossibility of securing a warm temperature in the room, which was situated on a damp earthen floor.—*Dublin Med. Press*, March, 1851.

29. *Extirpation of Neuroma without Injury to the Nerve*.—Formerly it was the habit to extirpate neuromatous tumours in totality, removing with them the portion of the nervous cord engaged in the tumour. The division of the nerve, when a large one, necessarily occasioned many inconveniences, as paralysis, contraction, &c. Subsequently, however, it was ascertained that the tumour involves the cellular tissue only, which envelops the nervous filaments, leaving the latter intact. Hence the evident indication that we ought, if possible, to dissect out tumours of this kind, and leave the nervous cord intact, or at least undivided. M. Bonnet, of Lyons, has performed an operation which shows the possibility of fulfilling the indication now laid down in cases where, some time ago, complete extirpation would have been resorted to.

A patient, aged 56 years, was affected with a tumour in the ham, arising from injury. The tumour had continued to grow slowly for nine years before it gave rise to any inconvenience. Some lancinating pains were then experienced, and the pains gradually augmented during two years, until they became insupportable. On examination, M. Bonnet found a tumour, about the size of a hen's egg, in the ham. Although the tumour presented pulsations isochronous with those of the heart, yet the pulsation ceased on pushing the tumour out of the trajectory of the popliteal artery. It was, therefore, probable that the tumour was merely neuroma of the popliteal nerve. Having first employed ether, M. Bonnet cut down on the tumour, and found that it was traversed nearly in the centre by the internal popliteal nerve. The tumour was now carefully divided, until at length the nerve appeared in sight. It was intact, and was separated without much difficulty from the tumour, which latter was enucleated from its sac and turned out. All pain, cramp, &c., now disappeared, and after the healing of the wound the patient was discharged perfectly cured.

30. *Congenital Obliteration of the Vagina and the Mouth of the Uterus*.—Dr. DEBROU, surgeon of the Hôtel Dieu of Orleans, has recorded (*Gazette Médicale de Paris*, Jan. 18th, 1851) a case of complete congenital obliteration of the vagina, with imperforation of the neck of the uterus, in which he re-established the vaginal canal and the orifice of the uterus. The patient subsequently married, became pregnant; and was delivered by the forceps of a dead infant. Labour was accompanied with convulsions, and followed by metro-peritonitis, and the patient died on the tenth day.

31. *Treatment of Stricture of the Urethra with Gum-elastic Catheters.*—[The *Dublin Medical Press* (April 16th, 1851) contains some interesting remarks on this subject, with a case by R. G. H. BURNES, Esq.]

John Clarke, æt. 40, a servant, was admitted under my care into Mercer's Hospital, February 28, 1851. His early life was very dissipated, and he refers to the fact of having had a succession of claps. So far back as twelve years ago, he was first seized with retention of urine, and relieved by the catheter; for three years before this he was labouring under great difficulty in passing water, frequent micturition, and many of the harassing symptoms of stricture. Dating from that period up to the present, he has had four attacks of retention of urine, which were relieved by instrumentation, warm baths, enemata, &c. &c.; while, during the intervals, he has been under the treatment of different surgeons, and generally with marked good effect.

Five months previous to the above date, he was seized with retention, and the urine drawn off by an eminent surgeon in the following manner: No ordinary-sized instrument could be got into the bladder, so he had recourse to the following procedure. The end of a long piece of catgut was made to traverse the contracted urethra fairly into the bladder, and over this, netting as a director, was slid a fine gum-elastic catheter; the catgut was then withdrawn, and the urine flowed off. During the consecutive five months from this date to the period of his admission, he never sought the aid of surgical advice, though for the last month the stream has never been thicker than a fine packthread, and very frequently the bladder has been emptied drop by drop.

On the night before this application to the hospital he had been drinking, and early in the morning was admitted with retention of urine. An ineffectual effort had been made to pass a catheter before I saw him, and I did not think it prudent to try again. By my directions a very full cathartic enema was administered, which acted freely. He was placed in a warm bath, and a full opiate given. When in the bath about a quarter of an hour, the urine began to dribble away, and the bladder emptied itself. I made no further examination then, but ordered a hip-bath at eight, and an oil draught with tincture of opium immediately after.

March 1st. The bladder has perfectly emptied itself, and I saw the patient make water to-day in a stream not thicker than a thread, accompanied by great straining. On examining the urethra externally, it is hard and firm to the touch, about two inches and a half from the orifice, and there is a firm, hard, unyielding mass behind the scrotum, in the perineum. On exploring the canal with a No. 7 bougie, it was abruptly stopped at the point above noted; instruments of various sizes were tried ineffectually, until a No. 1 gum-elastic catheter, mounted on a firm stilet, was made to pass. This grated along a firm unyielding structure, at least an inch in extent: the instrument then moved freely on, until in front of the hulk, when it met with the same kind of resistance as that which it had just overcome. This part of the urethra was so contracted, that with great difficulty the catheter was made to pass, and, having arrived at the membranous portion, it was arrested altogether. Having gained so much, I commanded the patient to let the instrument remain in as long as it did not produce much uneasiness there. He was able to wear it for two hours and a half. On its being withdrawn, ordered—

R. Mist. camph.  $\bar{\text{ij}}$ ;

Tinet. opii gutts. xxx;

Liq. potass. gutts. xv. M. Ft. hanst.

And immediately a hip-bath.

2d. Made trial of the No. 1 catheter again to-day, but with no better effect than on yesterday; it remained wedged in the stricture for three hours, and was horne without pain. The draught and hip-baths as on yesterday, and at night an oil draught with opium.

3d. The stream of urine passed to-day not enlarged, but attended with less straining. I passed the finest gum-elastic catheter made, and succeeded in getting it into his bladder. Retained it there in the usual manner, by means of an ivory ring and tapes. An opiate immediately, and a hip-bath at night.

4th. The catheter retained in the bladder all night; suffers no inconvenience from it; takes out the wooden peg occasionally to pass water. Repeat the anodyne draught and hip-bath at night.

5th. Yesterday, on getting into bed after going to stool, the instrument slipped out. I saw him immediately after, and by very gentle manipulation slipped in a No. 2 gum-elastic catheter firmly mounted, without giving the least pain; retained it as before; administered an anodyne immediately after, and ordered a hip-bath at night.

6th. Has suffered no pain from the presence of the instrument since yesterday; relieves the bladder through it occasionally, by removing the wooden peg: slight purulent discharge from the urethra, shewing the effect of pressure on the strictured parts. To have a hip-bath and anodyne at night.

7th. Slept all night; feels no uneasiness from the catheter, which has not been disturbed for forty-eight hours. Repeat the hip-bath and anodyne at night.

8th. Withdrew the catheter; slightly incrustated with lithic acid deposit, it being retained in the bladder seventy-two hours; has suffered no inconvenience from it; passed a No. 3 gum-elastic catheter firmly mounted; for a short time it was resisted at the membranous portion of the urethra, but in five or six minutes was allowed to glide into the bladder; retained it as before; administered an opiate immediately, and to have a hip-bath at night.

11th. Suffers no pain from the instrument, though undisturbed for the last seventy-two hours; withdrew it, incrustated with lithic acid deposit; purulent discharge from the urethra not increased; passed into the bladder a No. 4 gum-elastic catheter: retained it as before; after doing so, ordered at once—

R Mist. campb.  $\mathfrak{z}\text{i}$ ;  
Tinet. opii gutts. xxv;  
Liq. potass. gutts. xv. Ft. baust.  
An oil draught at night and a hip-bath.

12th. Feels very comfortable; bowels gently freed, without pain; no uneasiness referred to the bladder. Omit the opiate at night; the hip-bath to be repeated.

14th. Some uriac and pus passes at the side of the catheter, indicative of the dilatation of the contracted parts; introduced a No. 5 gum-elastic catheter to-day. Stopped the opium; hip-bath at night.

16th. Urethra and bladder so quiet, passed in a No. 6 gum-elastic catheter, and retained it as before; immediately after gave him opiate and alkaline draught; hip-bath at night.

20th. Last night the instrument slipped out; I tried this morning to pass it, and could not readily do so; therefore administered a full opiate, placed him in a warm hip-bath, and in an hour after, without the least difficulty, passed the same catheter into the bladder, and retained it there.

22d. Passed into the bladder a No. 7 gum-elastic catheter, and immediately after put him into a hip-bath, and gave a full opiate, and at night the bath to be repeated.

26th. Has had a hip-bath every night since last report, but the opiate was suspended; removed the No. 7 catheter from the bladder; though creating no irritation, yet I wished to substitute a larger one. This last was undisturbed for ninety-six hours; scarcely any purulent discharge from the urethra; the urine let off by it, four or five times in the twenty-four hours, is quite clear, deposits no sediment on cooling, which, taken together with the patient's feelings, point to, and substantiate the fact that there is no irritation of the bladder. Tried to pass a No. 8 gum-elastic catheter, but failed; administered an opiate, and had him placed in a hip-bath, and I returned in two hours, when I readily passed the No. 8 instrument into the bladder, and retained it there.

29th. Has not had the least annoyance since last report; the urine was quite natural in colour, and no mucus or sediment deposited on cooling. I withdrew the No. 8, and passed into the bladder a No. 9 gum-elastic catheter, and retained it as before. Hip-bath every night.

April 1st. Removed the No. 9 instrument and quickly introduced a No. 10

gum-elastic catheter, and fastened it as before. Hip-bath and an oil draught with opium at night.

2d. In going to stool this morning, the catheter slipped out, but without difficulty I replaced it. To have his hip-bath.

6th. Is not suffering the least pain or inconvenience from the presence of the instrument. There is merely a trace of pus from the urethra, and no evidence whatever of irritability of the mucous membrane of the bladder. On this morning I readily introduced a No. 11 gum-elastic catheter firmly mounted, and retained it as before; an opiate was given soon after, and a hip-bath ordered at night. On examining the urethra externally, the hardness and adventitious structure, deposited both anteriorly and behind the scrotum, are nearly all removed, and no pain whatever is elicited on pressure along the entire track of the canal. Not the least remarkable feature in the history of this case is the improved appearance and general health of the patient. He has lost the sallow, haggard look, and anxious countenance—those features so peculiar as to be almost pathognomonic of the affection under which he laboured; he has now pulled up flesh, and his entire appearance bespeaks happiness.

The mode of cure by the catheter—the practice adopted in this case—was introduced by the celebrated Desault, who made all strictures amenable to its employment. The treatment by this method has also met with warm supporters in the names of Brodie, Liston, and Miller. The former says: "When the gum catheter has entered the bladder, withdraw the stilet, and leave the catheter with a wooden peg in its orifice, which the patient is to take out whenever he has occasion to void his urine, it being at the same time secured by a suitable bandage. After three or four days, you may withdraw the catheter for twelve hours; or, if much supuration is induced in the urethra, you may withdraw it for a longer period; then introduce a larger catheter than the first; and thus you may, in the course of ten days or a fortnight, dilate a very contracted urethra to its full diameter. This is a very certain and expeditious method of curing stricture."—*Brodie, op. cit.*, p. 51.

Mr. Liston, at p. 472, *Practical Surgery*, expresses himself to this effect: "If the operation has been performed on account of retention, or if it has been threatened—and it is very apt to follow the swelling which always supervenes more or less upon the use of an instrument in very bad stricture—it may be prudent to retain the catheter. This is a very efficient, safe, and quick method of freeing the patient from bad stricture. It matters not how small the foreign body may be. Nature soon sets about a process to free herself from it; the passage is widened remarkably, and a most profuse discharge established, so that within forty-eight hours the instrument, which had been grasped most tightly, lies now quite loose, and the urine flows along it; it may then be withdrawn, and a large-sized catheter or bougie immediately substituted without difficulty."

Now, as to the two points of practice in my case, the wedging of the catheter in the stricture day after day until it reached the bladder, and then the retention of the instrument, and the substitution of larger ones, according to circumstances, until perfect dilatation was accomplished, are borne out by Professor Miller, of Edinburgh, in an admirable paper on the treatment of stricture of the urethra by perineal section, read before the Medico-Chirurgical Society of Edinburgh, and published in the *Lancet* for March 22, 1851.

"We must not lose sight," says this eminent professor, "of the two modes of using the catheter and bougie, well adapted to the final subjugation of cases even of great obstinacy. The method of *tunnelling*, as it may be called, founded on the fact that immediate penetration of the strictures is not essential to its cure. Instead of a small bougie, one of medium size is selected, and is passed down to the contracted part, into the anterior portion of which the extremity of the instrument, made somewhat conical for the purpose, is sought to be insinuated. There it is allowed to remain for a longer or shorter time, according to the feelings of the patient, and such use of the instrument is repeated at the ordinary intervals. On each occasion the penetration may be expected to deepen; ultimately the whole obstruction having been removed, as it were, by instalments, the instrument glides unopposed into the bladder, and, from this

high platform, the surgeon then proceeds in the ordinary work of final dilatation." "In my own experience, many an obstinate stricture has given way satisfactorily to this means, often without much or even any delay, and always without any unpleasant complication." "The other method is by tying in the catheter for forty-eight hours or thereby, a method well suited to the gristly and resilient stricture."

One of the striking features in the case just detailed is the fact that very little irritation was produced by the presence of the instrument, and this is the more remarkable when we bear in mind that the urethra was so narrowly contracted, in many inches of its course, as only to admit the finest catheter made; indeed so tight did it fit when it had reached the bladder, that it was with great difficulty moved either backwards or forwards. At the end of fifty hours, vital dilatation had taken place to such an extent that, when the patient went to stool, the instrument readily slipped out. Now, the increased calibre produced in the urethra was not attended at any period with very considerable amount of purulent secretion, and towards the end of the treatment it had nearly subsided altogether. During an uninterrupted period of thirty-seven days, this man had a catheter retained in his bladder, and through which the urine was voided for that time. By this means the fluid rarely or never came in contact with the walls of the urethra. To prevent such an occurrence, whenever the instrument felt loose, I always withdrew it and substituted a larger size, so as to prevent the urine trickling along its sides. There are other minutæ to be attended to in the local management, which are closely watched here, and though apparently trifling, I conceive of great moment in warding off irritation. The first is, not to allow the catheter to project far into the bladder; and secondly, to permit a small quantity of urine always to remain in the bladder. By these precautions, the instrument is prevented fretting the mucous membrane of that viscus. After changing the catheter, a full opiate was in every instance immediately administered, and the patient placed in a hip-bath; indeed, to the constant immersion of the parts under treatment in hot water, the free exhibition of opiates and gentle laxatives, I attribute mainly the exemption from irritation and rigors which so strikingly characterized this case all through. I have selected the foregoing case to show that the most unpromising and advanced callosous stricture may be brought to yield to the judicious application of the gum-elastic catheter—unfortunately a mode of treatment which, without good reason, has been allowed to fall into disuse.

32. *Congenital Dislocation of the Head of the Radius forwards.*—Prof. R. W. SMITH exhibited to the Pathological Society of Dublin a cast and preparation of this rare malformation of the elbow-joint, and made the following observations respecting it. In February, 1840, in a communication upon the subject of abnormal elbow-joints, Mr. Adams detailed the particulars of the case of a man named Horseman, whose right elbow-joint presented an example of congenital luxation of the head of the radius upwards and outwards, and exhibited a cast, showing the external characters of the deformity;\* and in March, 1849, he brought before the notice of the Society the subject of congenital luxations of the head of the same bone backwards. Upon the present occasion, through the kindness of my friend, Dr. Mayne, I have an opportunity of exhibiting a cast and preparation of congenital dislocation of the radius directly forwards. I may here mention that the female who was the subject of this deformity had likewise congenital dislocations of the wrist and of the knee; to these, however, I shall not now allude, but shall confine myself to the consideration of the condition of the elbow-joint. The woman was about forty years of age, and had been long an inmate of the South Dublin Work-house; she died of an attack of acute dysentery in the early part of 1849, and the following were the results of the examination of the left upper extremity:—

The forearm was flexed upon the arm at a right angle, beyond which it could not be bent, and could only be extended so far as to form a slightly obtuse angle. It remained habitually in the semi-flexed position, with the hand mid-

\* Dublin Medical Journal, first series, vol. xvii. p. 505.

way between supination and pronation; neither of the latter motions could be fully performed.

The joint, viewed upon its external aspect, presented a remarkable projection, formed by the outer condyle of the humerus, which descended so low as to be, in the semi-flexed position of the joint, nearly upon the same level as the commencement of the olecranon process of the ulna; it was, moreover, curved forwards and inwards, so as to present posteriorly a striking convexity; it was much larger and more prominent than the internal condyle, which, however, also seemed to be placed lower down than natural with respect to the ulna.

The summit of the olecranon, in every position of the joint, was placed above the condyles; but its lower portion, where it springs from the shaft of the ulna, was in a great measure concealed between these processes.

Directly in front of the enlarged outer condyle of the humerus, a rounded osseous tumour could be felt, which, as it partook of every motion imparted to the shift of the radius, was concluded to be the head of that bone; as much of it as could be felt was of an orbicular form.

Comparing, in my mind, these appearances with those observed in the case of Horseman, whose elbow-joint I had frequently examined, and who was known never to have received an injury, nor to have suffered from disease of the articulation, there was no difficulty in arriving at the conclusion that the case afforded another example of congenital dislocation of the head of the radius; and the opinion that the luxation was congenital was further confirmed by the co-existence of the malformations of the right upper end of the left lower extremities, which I have already mentioned. There exists, however, this difference between the two cases, namely, that, in the present instance, the head of the radius is displaced forwards, while in that of Horseman, it is described by Mr. Adams as being dislocated upwards and outwards.

The muscles (which, although pale, were not remarkably attenuated) having been removed, the bones and ligaments were carefully examined. The lower extremity of the humerus presented no traces either of trochlea or capitulum, nor any evidence of these processes having ever existed. In place of them there existed a deep fossa or excavation, into which was received the great sigmoid cavity of the ulna, in such a manner that, when the forearm was flexed to a right angle, the coronoid process struck against the front of the humerus, and extension was at once checked by the olecranon coming in contact with the back of the same bone.

The outer condyle of the humerus, which was much larger than natural, and curved forwards, was deeply excavated anteriorly, in such a manner as to form with the lesser sigmoid cavity of the ulna, which was also enlarged, a socket which accommodated the head of the radius, which was strangely altered from its natural form.

This process, instead of presenting a circular outline with a concave summit, resembled in form the section of a sphere, its internal portion being, as it were, cut vertically, so as to present a surface nearly flat to the altered lesser sigmoid cavity of the ulna; the rest of the head of the bone, namely, all that could be felt through the muscles and integuments, was of an orbicular shape (as in the case of Horseman), and rolled in the excavation of the humerus already mentioned, during the motion of supination, which could be carried to a much greater extent than that of pronation. Both these motions were, however, very confined, for the lower extremity of the radius was as ill adapted to their complete execution as the superior. The lower extremity of the radius, which, in the natural state, presents a concavity where it articulates with the head of the ulna, was in contact with the latter by a nearly flat surface.

The superior extremity of the ulna was twisted in such a manner that the cartilaginous surface of the greater sigmoid cavity of the ulna was directed inwards. The neck of the radius could not be said to exist, the head springing almost directly from the shaft of the bone; the reverse of what is usually found in cases of congenital luxation of the head of the radius backwards, the neck of the bone being in these cases elongated to such a degree as to render the radius and ulna of equal length. The external and internal lateral ligaments existed, but ran in a direction nearly horizontal; the former passed outwards in

an almost transverse course, to be attached to an exceedingly thin, broad, and imperfect coronary ligament.

The similarity which this case bears to that detailed by Mr. Adams will be rendered evident by the following extract:\* "When we view the joint on its external aspect, the outer condyle is observed to be large, and placed as low down nearly as the olecranon process; above it a very conspicuous orbicular eminence is seen, which moves freely with the radius when a motion of rotation is communicated to the forearm; the inner condyle also descends very low down; the two condyles and the lower portion of the olecranon process (in the semi-flexed position of the joint, which it ordinarily remains in) are on the same plane; the lower extremity of the humerus is probably excavated to receive the greater sigmoid cavity of the ulna. The neck of the radius rotates on the ridge of the humerus, which descends to the outer condyle, above and somewhat behind the plane of the most anterior part of which the head of the bone is placed." Mr. Adams remarks, in conclusion, that this is the third case of lateral dislocation of the radius recorded, and the first of the congenital luxation of this kind as yet exhibited to the profession.

In the same year, that is, in 1840, Guérin recorded a case of congenital luxation of the head of the radius forwards and upwards,† but the record is so brief and so completely deficient in details as to be of little value. It is as follows: "*Luxation de la Tête du Radius en avant et en haut, consistant dans le glissement de cet os au-devant de l'humérus, vers la fossette coronoïde de ce dernier. Cette luxation est nécessairement accompagnée de diastase des articulations radio-cubitales et de pseudo-luxation du carpe. Nous avons eu, l'année dernière, dans notre service, une jeune fille de sept ans offrant la même difformité des deux côtés.*"

Three varieties of congenital luxations of the head of the radius have now been established, viz: backwards and upwards; forwards, upwards, and outwards; and directly forwards.—*Dublin Quarterly Journ. Med. Sc., Aug. 1850.*

33. *Dislocation of the Clavicle downwards, beneath the Acromion Process of the Scapula.*—R. G. W. BUTCHER, Esq., records, in the *Dublin Medical Press* (April 9, 1851), a case of this very rare accident, in a powerful muscular man, 50 years of age, a patient at Mercer's Hospital.

The mode in which the accident occurred was the following: The man was assisting a comrade in carrying a heavy press, the edge of which rested on his right shoulder. The load was too great for him, and he yielded under it, the weight changing its position farther in upon the clavicle; in falling, he thrust his hand out rigidly against the ground in the effort to save himself; this checked his fall, but from the resistance offered, the humerus, with the scapula fixed upon it by its powerful capsular muscles, was driven forcibly upwards, while the weight still acted on the clavicle, tearing through its ligaments and impelling it downwards: thus the two forces, tending in contrary directions, combined towards the production of this rare displacement. On removing the clothes from the upper part of the body, and viewing the patient in front, the most striking characteristic was the non-symmetrical distances between the external margin of the acromion processes on the right and left sides and the mesial line of the sternum; that on the right being, by measurement, an inch and a quarter less. The right shoulder also presented a very remarkable prominence, formed by the acromion, and rendered more in relief by the depression internally, occasioned by the clavicle passing beneath it; the lip of the latter could be felt a little to the outer edge of the external margin of the acromion, and lifting up the fibres of the deltoid; the superior edge of the articulating surface of the acromion was elevated, and might be discerned through the integuments; the fingers could be passed uninterruptedly from behind forwards along the spine of the scapula to its expansion into the acromion process. On pressure at its outer side, the projecting articulating end of the clavicle beneath it could be detected, as noticed above. The rotary motions of the joint were perfect, and might be performed by the surgeon without pain or suffering; but on the part of the patient there was complete inability to elevate the limb.

\* Loc. cit.

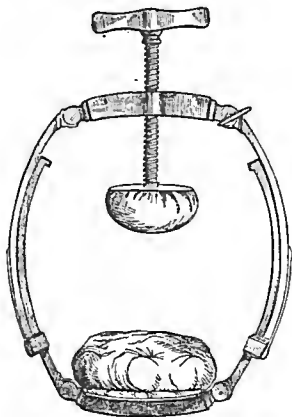
† Gazette Médicale, 1840.



The deformity was removed when the shoulder was drawn forcibly back, or by the following manœuvre—first, drawing the arm a little backwards, keeping the extremity of the humerus close to the side with the right hand, and with the left grasping the upper part in the axilla, then, by pressing the shaft of the bone upwards and outwards, and at the same time carrying its inferior extremity forwards in a line with the anterior wall of the axilla, and continuing the force upwards, the acromion process was disengaged from off the clavicle, and by using a pad in the axilla, and retaining the arm in this position, excellent coaptation was effected. Mr. B. saw this patient three times after the accident, at intervals of a fortnight. After the pain and uneasiness about the joint had subsided, he became intolerant of restraint; and in five weeks used the limb with nearly as much freedom as the sound one, but with considerable loss of power.

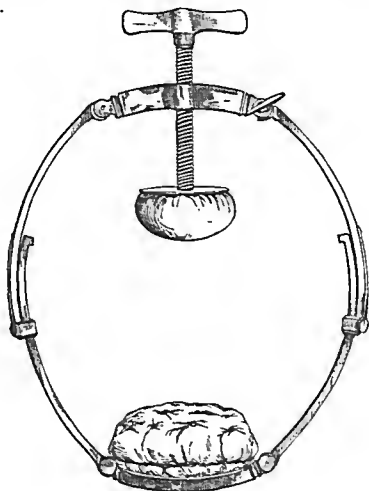
A well-described case of this accident is given by Tournel in *Archives Générales de Médecine*, 1837; and another has recently been treated by Mr. Partridge, in King's College Hospital, London, and alluded to in the *Lancet* for January, 1850, in these terms: "The unusual circumstance of the case is that the acromial end of the clavicle is dislocated, not upon the acromion, as is commonly the case, but *below* that process, a fact which the fingers easily detect. There is no fracture, as might at first have been imagined, but this rare displacement has taken place, probably owing to the peculiar manner in which the horse or vehicle struck the patient's chest."

34. *Skey's Tourniquet*.—The tourniquet which I have substituted is composed of two semicircles, one of which fits into the other by running in a groove. Each half is fixed by a spring catch to the other, and may be enlarged or reduced at will to any size required for the thigh or upper arm. When required for application to the thigh, the circle, which is made to open, to admit of its application around the limb, is drawn out to its fullest size. In the centre of



each semicircle is the pad for pressure and counter-pressure, the former being provided with the ordinary screw. The pads are made small, in order to include as little surface in the pressure as is compatible with the safe application of the instrument. When employed for a lesser limb, the arm for example, or

the thigh of a child, the circle is lessened to the required size by raising the lateral springs, and pressing the outer half or semicircle downwards upon the inner one, by which the large circle is converted into one of smaller size, the alteration being obtained by the introduction of two hinges in each half of the instrument.



I have found this tourniquet exceedingly efficient in its avoidance of the objections to that commonly used, and equally so in its power of arresting arterial hemorrhage. It is also especially applicable to cases requiring pressure for the treatment of aneurism, being less irksome and painful to the patient, when worn for a considerable time. The circulation in the main artery, which is the only one demanding compression, and for which purpose alone, indeed, the instrument is worn, is thoroughly arrested, while the collateral vessels are unaffected, and the current of blood in the venous system, except that of the chief vein, which is in close proximity with the artery, is altogether uninterrupted.—*Operative Surgery.*

35. *Injury of the Shoulder-Joint.*—Dr. ANAXES described to the Pathological Society of Dublin a case of dislocation of the head of the humerus forwards, which had been reduced, with the account of the *post-mortem* examination of the articulation, made thirty days after the occurrence of the luxation, the patient having died from the effects of another injury sustained fourteen days before her decease.

Eliza Dillon, aged 65, applied for surgical aid at the Richmond Hospital on October 27, 1849. She stated that she had fallen with much violence, and with her left arm extended from her side, in such a manner that the shock was chiefly sustained by the inner part of the elbow. The resident pupil, Mr. Johnston, recognized the usual characters of dislocation of the head of the humerus

forwards; he noticed a remarkable flattening of the deltoid muscle, and felt the head of the bone in its new situation. His colleague, Mr. Roden, also recognized the injury, as did likewise Surgeon Bichaque. The head of the bone was replaced without difficulty, Mr. Johnston having adopted the plan of placing the knee in the axilla; the patient expressed herself relieved, and went home. In a few days she returned to the hospital, complaining of more pain in her shoulder-joint than patients usually do after the reduction of this dislocation, when she was subjected to the ordinary treatment for a sub-inflammatory condition of the articular structures.

On the sixteenth day after the shoulder had been luxated, this woman, being deaf and rather feeble, while heedlessly walking across the street, was knocked down by a passing vehicle, and received a compound fracture of the elbow-joint, of which, after having suffered from constitutional irritation and erysipelas for fourteen days, she died. The left shoulder-joint was examined after death: the deltoid muscle having been reflected, it was seen that the supra and infra-spinatus muscle had torn off a large portion of the great tuberosity of the humerus, and that this portion had been partially reunited to the shaft by recent bony deposit. The anterior portion of the tuberosity, which lay next to the bicipital groove, still remained entire, and gave attachment to some of the tendinous fibres of the posterior articular muscles already mentioned.

Upon exposing the subscapular muscle, its upper margin, near to the coracoid process, seemed somewhat ecchymosed; and upon lifting up this muscle from the subscapular fossa, in the vicinity of the coronoid process, some well-formed pus was evacuated from the interior of the joint.

The capsular ligament was of a yellowish-white colour, and thickened, but no rupture could be discovered in it. On cutting into it, the whole of the synovial membrane was found to be in a high state of vascularity. The appearance which it presented might be compared to that of the conjunctiva when in a state of acute inflammation. Upon examining the glenoid cavity, it was discovered that a portion of its inner margin was torn off. This piece of bone was more than half an inch in length, and three lines broad. The cartilage of incrustation of the head of the humerus presented a porous appearance, as if punctated by small pin-points, and was very thin, as in some examples of diffuse inflammation.

With respect to the muscles in this dissection, it is to be remarked that they seemed to have suffered no injury. The supra-spinatus and neighboring short articular muscles are those which in general have been found to have suffered most in cases of dislocation of the shoulder; although in this instance these muscles were not torn, their tendinous insertion had been partially broken off. The double lesion of the tuberosity of the humerus and inner margin of the glenoid cavity, if it had been to any greater extent, might have prevented the head of the humerus from being preserved in its socket after the reduction of the displacement, and thus have presented a repetition of a specimen preserved in our museum, and which Mr. Smith has already laid before this Society. In the case I have brought before the meeting, the bone, once replaced, remained in its socket; but in Mr. Smith's case, as the tuberosity was entirely torn off, all the posterior articular muscles, which should have retained the bone in its place when restored, had lost connection with the humerus; it was thus abandoned altogether to the force of the muscular folds of the axilla, which displaced the bone forwards and inwards, and held it in the abnormal position. Mr. Thompson, about the year 1792, first noticed that a dislocation of the humerus might be complicated with a fracture of the tuberosity; and Sir Philip Crampton published a similar case in the third volume of the First Series of the *Dublin Medical Journal*.

With respect to the capsule in Sir A. Cooper's and Sir Philip Crampton's cases, this ligament was extensively torn. In the case now adduced, examined thirty days after the injury, no rupture of the ligament could be observed. We may, I think, from this infer that, in this case, notwithstanding the obvious deformity observed, the degree of displacement must have been to the smallest amount possible, as the capsular ligament had not been torn; yet there can be no doubt but that the head of the dislocated bone lay beneath the root of the coracoid

process. It is true the laceration of the insertion of the posterior articular muscles may have allowed the posterior portion of the capsular ligament to yield somewhat, and the tearing off of the inner or anterior margin of the glenoid cavity may have contributed to the same result.

Amongst the Hunterian manuscripts, were found, in the handwriting of John Hunter, the notes of a case of dislocation of a shoulder-joint, which resembled this in many particulars. A man was admitted into St. George's Hospital, London, with a dislocation of the humerus into the axilla, which was reduced, and about three weeks afterwards he died of fever. "I was anxious," says John Hunter, "to see the state of the parts in so recent a dislocation. On dissecting off the infra and supra-spinatus muscles, I found nothing uncommon respecting the capsular ligament; but when I dissected off the subscapularis I found the ligament in some degree injured, so as to have lost a good deal of its uniformity. It was of a dark bluish colour, in consequence of extravasation of blood into it, in part absorbed. I cut the capsule round nearly to the os humeri, where it was sound, so as to expose the inner surface of the ligament, and found that on the inner surface between the insertion of the tendon of the subscapularis muscle into it and its fixture to the edge of the glenoid cavity, it was injured, corresponding to the external surface, although I could not say fairly ruptured through. I also found that a circular part of the cartilage on the edge of the glenoid cavity, to which the ligament is attached, was torn away from the bone for about an inch of the circle, and which must have been pulled off when the head of the humerus pressed against the ligament with great force; but it kept its attachment by the remainder. The separation was such that it could not allow anything to pass between the bone and it. There was little or no extravasation of blood in the cavity of the joint; but, what was very remarkable and what I did not expect, I found a good deal of pus in the joint. If this is common in such cases, what becomes of it? Here," he adds, "was a case of *undoubted* dislocation, and yet the capsular ligament was *not* torn when the luxation was effected, although it must be supposed that the giving way of the cartilage at the edge of the glenoid cavity admitted the ligament to yield more than it otherwise would. Although the ligament was not torn so as to let the head of the bone escape through the rent part, yet the head of the bone was certainly out of the socket; and, from all the appearances and circumstances taken together, how it got there without doing more mischief I do not understand."—*Catalogue, Royal College of Surgeons, England*, vol. ii. p. 206.

In visiting the museum of the College of Surgeons of London, two years ago, I took particular notice of this preparation thus alluded to in the catalogue; and I at the same time felt astonished that these facts, recorded by John Hunter himself, had escaped, as far as I knew, the notice of those who had written on dislocation of the shoulder. This case recurred to my mind with much interest when I met with the specimen I now present to the Society. From such cases as these we should infer that we ought to examine carefully every case of luxation of the shoulder-joint, both before and after we have replaced it, with a view of ascertaining if the luxation is a simple one, and unaccompanied by fracture or lesion of bone besides the luxation; and, secondly, that we should watch any symptoms of arthritis in time, in order to meet them with appropriate treatment, and prevent any serious consequences. In the case I have presented we discovered an amount of inflammation of the shoulder-joint, which no one had anticipated to have existed at the time when the patient was carried off by the effects of another injury.—*Dublin Quarterly Journ. Med. Sc.*, Aug. 1850.

36. *Fragilitas Ossium*.—Prof. R. W. SMITH exhibited to the Pathological Society of Dublin the pelvis and thigh bones of a female who had suffered from this disease, and which had been forwarded to him by Dr. Campbell, of Lishurn. The following was the history of the case, as furnished by Dr. Campbell:—

Eliza Cosgrave, about forty-five years of age, a married woman, and mother of two children, the elder being nine years old and healthy, the younger having died when about three months old, began, shortly after the birth of her second child, to complain of pains in her limbs, and generally over her body, which she attributed to her residence in a damp house. She soon became so helpless as

to be unable to get into or out of bed without assistance. On one occasion, while being helped into bed, her thigh was struck against the bed-post, and the femur broken just below the trochanter. She now obtained admission into a neighbouring hospital, where she remained many months, and was then discharged without having experienced any amendment of her condition. In this state she was removed into the Lishurn Union Workhouse, about two years ago. On examination, the injured limb appeared to be about three inches shorter than the other; there was no crepitus, although acute pain was experienced when the thigh was handled or moved. Her general health at this time did not appear much broken, and her appetite was good; but her pains, chiefly in the thigh, were so severe as to require the administration of an opiate every night. Several months having elapsed in this manner, one night whilst the nurse was turning her in bed, the other thigh was also broken near the trochanter, after which her pains for some weeks were mitigated to a certain extent. Diarrhoea at length set in, and resisted all treatment; her pains returned with greater violence than before, and she died after a few weeks of extreme suffering.

Upon examination after death, the fractures were found to have occurred about two inches below the great trochanters; they had both become consolidated, but with great deformity remaining, the fragments being, upon each side, at right angles with one another. The pelvis and thigh bones were so light as to float upon water, and so fragile that a slight pressure of the finger was sufficient to crush the osseous tissue. The compact structure of the femora was as thin as an egg-shell, and the medullary canals enlarged, here and there crossed by delicate osseous septa, and filled with a grumous semifluid substance, resembling a mixture of medullary matter and blood. Mr. Smith observed that the facility with which fractures united in such cases was remarkable, the union, as Mr. Stanley has noticed, occasionally taking place within the ordinary period. Mr. Tyrrell has recorded, in the "Reports of St. Thomas' Hospital," a case of fragilitas ossium, in which twenty-two fractures occurred, and observes that the injuries were repaired with greater rapidity than he had seen in other individuals, the union of the fracture of the femur being perfectly firm at the expiration of three or four weeks.

Mr. Smith, in conclusion, alluded to the extraordinary case of this disease recorded by Saillant\* in which the lightness of the osseous system was such that the patient, an adult female, when placed in a warm bath, actually floated upon the surface of the water.—*Dublin Quarterly Journ. Med. Sc.*, Aug. 1850.

37. *Chloroform in Gonorrhœa*.—M. VESOT recommends injections of chloroform as an abortive treatment of gonorrhœa, that is to say, of arresting it in the earliest period of its development; and thinks that it will replace the nitrate of silver, which is so frequently employed for this purpose. Pure chloroform is injected by a glass syringe, the perineum being pressed upon; the first effect is burning heat, then a sensation of cold follows. The injection does less good after the first two or three days of the gonorrhœa have passed; but, employed before that time, it almost invariably arrests it. Injected into the vagina, the results were less satisfactory.—*Med. Times*, April 19, 1851, from *L'Union Méd.*

## OPHTHALMOLOGY.

38. *Observations on Collyria*.—[Dr. ARTHUR JACON, one of the best informed, most judicious, and most reliable ophthalmic surgeons of our day, has published, in the *Dublin Medical Press* (May 7th, 1851), some very interesting remarks on collyria, which are well worthy the consideration of practitioners:—]

While discussing the treatment of each form of purulent ophthalmia, and the consequences thereof, I have had to notice repeatedly the various medicinal remedies applied directly to the conjunctiva, and to suggest inquiries respecting their *modus operandi*. I find, however, that it is desirable to offer some addi-

tional remarks on the subject by way of explanation. I have said that these remedies probably operate in three ways: as stimulants, astringents, and escharotics; and, I may add, as sedatives. This, I think, can scarcely be denied. That most, if not all of them, stimulate, cannot, I think, be doubted, and that some act as escharotics, is equally obvious; the astringent effect also seems undeniable, although perhaps the exact change of structure produced by astringents may not be so clearly established. It is assumed to be a diminution of the size of capillary vessels and a contraction of secreting parts, and apparently with reason. The sedative effect is not, however, so unquestionable, if we assume that the cessation of pain is evidence of it; for, notwithstanding an apparent belief to the contrary, remedies which uniformly allay pain are not in our hands. Still the existence of these four qualities in medicinal remedies, in a greater or less degree, is so far established that the practitioner may with safety make his selection in accordance with such a view. The use of an application simply stimulating in inflammation of the conjunctiva, or its consequences, may on consideration appear difficult of explanation; it seems like adding fuel to fire; yet it is doubtful whether the beneficial effect of a solution of nitrate of silver at this period is not from its stimulating properties: the impunity, if not advantage, often derived from weak medicinal solutions, indiscriminately used in routine practice, is also in favour of the conclusion that, even in the acute or active stage of inflammation, stimulation may sometimes be beneficial. I am not, however, advocating the practical application of such a theoretical view, I would only suggest an inquiry into its soundness; for the subject is one for consideration in other departments of the healing art as well as this. But whatever doubts may be entertained respecting the possibility of a beneficial operation by stimulants in the acute stage, there cannot, I think, be any respecting the advantage to be derived from them, sometimes at least, in that state of the conjunctiva which follows, and which has been called the chronic stage. After the tension of inflammatory action subsides, and when pain abates, and a mild purulent secretion is established, astringent applications unquestionably prove beneficial; and at a more advanced period, if these fail, or that symptoms demanding them ensue, stimulants also are found of advantage. The peculiar condition of the parts to which stimulating applications are suited does not, however, appear to be well understood; they are often used indiscriminately or empirically. It is, I think, when a sensation of scalding, with flow of tears and intolerance of light prevails, that they are useful; or when no other inconvenience remains except a certain tenderness or rawness of surface, depending perhaps on a want of epithelium. Of stimulating applications, the two most extensively employed are the *vinum opii* and the weak solutions of sulphate of zinc and copper, and such has been their success that they have become popular remedies, even beyond the limits of our profession.

I have already in a former paper expressed my opinion as to the value of the *vinum opii*, and quoted Mr. Ware's account of its benefits in his practice. It is now more than fifty years since his observations were published, and I know of no local application which has maintained its character amongst practical men so well. It often fails, it is true, especially when used in cases unsuited to it; but all men of experience agree that it is frequently a most useful application. Notwithstanding, however, the evidence in support of its beneficial operation, strange to say, an attempt has been just made to discard it from practice by omitting it from the Dublin Pharmacopœia, and substituting for it a simple vinous tincture without any of the aromatic stimulating ingredients. The reasons for this change I cannot discover. It was, I believe, first suggested by an oculist in London in extensive practice, although an ephemeral writer for the less scrupulous journals takes credit for its adoption. Whatever benefit attends its use seems to depend on its stimulating properties quite as much as its sedative, but probably it arises from the union of the two, although such a combination may appear unaccountable. At all events, the tincture substituted for it has no particular value, and must from the variable nature of the menstruum, and its liability to adulteration, be a very unequal preparation. Indeed, the beneficial effect of simple solutions of opium, its ingredients or pro-

ducts, in allaying painful sensibility of the conjunctiva and diminishing vascular turgescence, seems not to be so obvious as might be supposed from the sedative properties of the drug. In one time made some experimental trials with them, using the strongest solutions in water and the common alcoholic tincture rendered less stimulating by the application of heat, but cannot say that the trial proved satisfactory. As I have stated in a former paper, the true *vinum opii* should be used in those cases only where scalding pain, profuse flow of tears, and intolerance of light prevail; and should now be prescribed under its old title of *tinctura thebaica*, or as the *vinum opii* of the London or Edinburgh Pharmacopœias.

Of the saline stimulants, the sulphate of zinc seems to have maintained its character above all others. It is the popular "eye-water" of this country, and could not, I think, have continued so for such a length of time unless it had been positively beneficial in a majority of cases. The sulphate of copper may, however, be of equal value, as well as the *lapis divinus*, a salt resulting from the mixture of alum, nitre, and sulphate of copper. Indeed, many other salts are probably equally efficient in proportion to their stimulating properties; and hence the popular use of vinegar and water, brandy, Eau de Cologne, salt and water, and even urine, which is sometimes employed notwithstanding the objections its use must suggest. The sulphates of zinc and copper and the *lapis divinus* may be prescribed in water impregnated with camphor, and in the proportion of about two grains to the ounce. I generally order it in the common camphor mixture of the shops, adding a drachm of spirit and five drops of dilute sulphuric acid to an eight-ounce bottle. I have, however, to repeat, that it is in chronic cases, where the conjunctiva and edges of the lids are raw and tender, whether from preceding inflammation or not, that these stimulating applications prove beneficial. While any inflammatory action remains, or a tendency to relapse prevails, they are not desirable.

Respecting the use of astringent applications, in both the acute and chronic stage, I have already expressed my opinion. I have now only to suggest that the practitioner will endeavour to act upon some definite and exact principle as to their use. He has to settle what the precise change is which the agents called astringents produce; then whether such change is beneficial in conjunctival inflammation or its consequences; and if so, what astringent should be employed. The precise change, however, has not perhaps been exactly ascertained; whether chemical, physical, or vital. All that seems to be proved is that the surface to which an astringent is applied becomes shrivelled or corrugated, whether living or lifeless; but whether vessels become diminished in calibre, or secreting structures disabled from acting, or the epithelium coagulated by its operation, remains to be determined. Be the change, however, what it may, it seems to be proved by experience that astringents diminish the enlargement of vessels caused by inflammation, in fact remove the increased vascularity or redness, while they, at the same time, diminish or arrest the secretion of purulent matter, and perhaps henuh the sensitive villi or papillæ. Of the medicinal agents used for this purpose, tannin from the organic, and alum from the inorganic sources, seem most entitled to confidence, considering their action on lifeless structures: hence the occasional use of infusions of galls, oak-bark, and other vegetable materials, and of solutions of tannin itself. I cannot, however, say that I have found tannin beneficial in the chronic vascularity of conjunctival inflammation. Its use seems to be revived and announced as a novelty by some Belgian practitioner, but I have not repeated the trials I formerly made of it. A saturated solution of alum (*sulphas aluminæ et potassæ*) may be dropped with safety between the eyelids once or twice in the twenty-four hours, and a weaker solution, five grains to the ounce of water, may be more freely used as a *collyrium*. It has, probably, more astringent with less stimulating properties than any other salt, but the sulphates of zinc and copper are also astringent, although more irritating. Acetate of lead seems also to be powerfully astringent, with little stimulating qualities, and even perhaps with some sedative operation. Nitrate of silver also, as I have already stated, is a valuable astringent in the proportion of ten grains to the ounce of water. The practitioner may, I believe, with safety, give most of the metallic salts a trial,

if so inclined, and may add to our information by recording the results of such experiments. At one time I made many experiments with some of them, but the result was a greater reliance upon alum, acetats or diacetate of lead, and nitrate of silver.

Of the value of what are called sedative applications in conjunctival inflammation and its consequences, no very accurate estimate seems to have been formed, notwithstanding their frequent use. Warm stupes and tepid lotions containing narcotic materials are considered beneficial in the acute stages; but whether the apparent advantage attending their employment depends more on the warm moisture than the medicinal influence, appears doubtful. I have from time to time used opium, hops, hemlock, hyoscyamus, belladonna, and tobacco, in this way, but still remain undecided as to their medicinal value, except in cases assuming a neuralgic character, where they appear to be of positive benefit when used of proper strength. The value of sedative applications directly to the conjunctiva seems also undecided, although generally assumed to be unquestionable. Opium, and the narcotic products derived from it, after a fair trial, seem beneficial in particular cases only. They do not uniformly allay the scalding pain which accompanies the increased vascularity consequent to inflammation, notwithstanding the notion very generally entertained that they must necessarily do so. I have just alluded to the use of the *vinum opii*, and am convinced that its admitted beneficial operation depends upon the combination of sedative and stimulant properties in the preparation; neither strong solutions in water, nor simple tinctures in wine or alcohol, have the same effect. I have tried tinctures of hop, hemlock, and tobacco, in cases where the *vinum opii* generally succeeds, but I cannot say that I have found them of much value, although they may be used with safety. The acetate and diacetate of lead are generally considered to possess sedative properties, but I cannot discover on what grounds. They are, I believe, powerful astringents, with moderate stimulating properties, and may be used in any strength, not being escharotic, however applied. The common Goulard's extract (*liquor plumbi diacetatis*) and the saturated solution of the acetate of lead may be dropped into the eye without fear of any ill consequence, and, in the chronic vascularity following catarrhal ophthalmia, with great benefit. I have been in the habit of using a *collyrium* made by mixing the diacetate of lead with the *aectum opii* in water; and latterly have been trying a strong preparation of this nature, made by mixing equal parts of Goulard's extract and *aectum opii*. There is of course decomposition in such a mixture, and a copious precipitate is thrown down; but the fluid obtained by filtering or rest is evidently an efficient one, and not irritating. I drop it freely between the eyelids, when the *villi papilla*, or glands of the conjunctiva lining the lids, are turgid or enlarged.

Of the use of applications positively escharotic, in both the acute and chronic stages of purulent ophthalmia, I have already expressed some opinion; and have now only to suggest a consideration of the nature of an escharotic so administered. It is, I conclude, assumed to be an agent operating as a chemical solvent of the structure to which it is applied, and being so, must be very cautiously used. I am not here alluding to its use in progressive ulceration of the conjunctiva, or to granular lids, but to its use as a dressing to the entire conjunctival surface; and as such, its true effect should be well understood. Here, however, a question arises as to the fact of escharotics being so used at all, and it must be admitted that, unless strong solutions of nitrate of silver are of this nature, few other solutions used, if any, operate in this way. Saturated solutions of alum or acetate of lead are certainly not chemically escharotic; neither perhaps are saturated solutions of sulphate of zinc or sulphate of copper, but strong acids, alkaline solutions, and lime suspended in water, are; although not medicinally used. I have said elsewhere that a solution of nitrate of silver containing thirty grains or less in the ounce of water, is escharotic, because, if repeatedly brushed over the surface of an everted eyelid, a gray pellicle is produced; yet such a solution may be dropped into the eye without burning the conjunctiva, and perhaps even a stronger solution might be so applied without destruction of surface. The ointment recommended by Mr. Guthrie, being ten



grains of nitrate of silver triturated with a drachm of lard, is also probably escharotic; although more or less of the nitrate must be decomposed by the addition of fifteen drops of diacetata of lead added.

I have here, as well as in other places, been endeavouring to direct the attention of practitioners to a more rational practice in the use of local applications to the conjunctiva; because, notwithstanding the length of time they have been used, they are generally administered empirically, and without a proper consideration of their exact nature. The subject is one which, perhaps, might be more effectually handled in a distinct essay than in a practical treatise, because it is one of importance in relation to other departments of surgery; but if it leads practitioners to reconsider their opinions respecting the comparative value of *collyria*, the inquiry cannot fail to be beneficial. While alluding to the subject, it may, however, be useful to remind them that these direct local applications may be often not only unnecessary, but absolutely mischievous. I have no doubt that recovery is often interrupted by the use of them, and that many of the worst cases of what is called chronic ophthalmia are reduced to that condition by premature and injudicious irritation in this way. It is true that, in general practice, the prejudices of the people do not permit a fair trial of less active interference, but in hospital and dispensary practice, and especially in the military department, an opportunity of doing so is afforded. Of the method of applying *collyria*, lotions, or solutions to the conjunctiva, it is necessary to offer a few observations, the importance of a proper administration of remedies in this way being generally overlooked. A drop of fluid placed on the surface of the eye, between the two margins of the eyelids, must have but a very limited operation, narrowed as the exposed surface is by the contraction of the orbicular muscle, and confined as the fluid is from spreading between the lids by the accurate fitting of the palpebral cartilage to the eyeball. To secure a full exposure of the whole conjunctiva of both the eye and the lids to the contact of the fluid, it should be introduced beneath the upper lid at the outer angle by separating the two surfaces and introducing a large drop from a full-sized camel's hair pencil between them. By laying the thumb or finger on the skin between the lid and brow, and pushing it up over the margin of the orbit, a space is made between the lid and ball, into which the fluid should be dropped, and from this it will spread over the whole surface. If a more complete saturation of the upper lid is required, it must be everted and repeatedly brushed over with the solution.

39. *Fungoid Tumour of Orbit—Extirpation—Death.*—Mr. J. B. FIFE presented to the New Castle and Gateshead Pathological Society (Nov. 9th, 1850) a fungoid tumour of the orbit which he had removed, a few months previously, from a child *æt.* 4, who was first noticed to be blind a year before the removal of the disease. At this time a bright metallic spot was seen at the depth of the eyeball. Little disturbance of the health took place for several months after this, and the eyeball remained in much the same condition until two months previous to the operation, when it began sensibly to project from the orbit, and to be inflamed and painful. The eyeball from this time was rapidly pushed out of the orbit by a lobulated, soft, fungoid tumour, and in two months projected beyond the edges of the orbit with the eyeball on its anterior surface. The child suffered great pain, and as the friends were wishful to have an operation performed, it was removed in the usual way: the hemorrhage, which was profuse, was restrained by plugging the orbit firmly. The wounds of the eyelids and of the orbit healed kindly, and the patient progressed favourably until three months after the operation, when, without any external signs of the return of the disease, the child was seized with convulsions, and died suddenly—no doubt from an extension of the disease in the cranium. The tumour was of a soft, spongy, fungoid consistence, devoid of any appearance of fungus hæmatodes, and consisted almost entirely of small granular and nucleated cells, contained in the meshes of a soft, cellular, fibrous-like stroma, which, on maceration, readily allowed the cells to escape, and the tumour to collapse to a very small size. The cells were round or oval, about half the size of pus-globules, all of them preserving a pretty nearly equal size, and possessed of one or more

nuclei, and numberless minute shining granules, both in the cells, and floating in the fluid containing them. The fibrous stroma consisted of fibres, many of them containing nuclei, more or less resembling caudate cells. The tumour evidently sprang from the optic nerve, and appeared to have passed along it into the optic foramen. The interior of the eyeball was filled with a thick, white, paste-like fluid, which consisted of cells similar in all respects to those compassing the tumour, presenting, however, no trace of fibres, and occupying the position of the tumours which were altogether absorbed.—*London Med. Gaz.*, Feb. 1851.

40. *Melanotic Tumour of the Orbit removed.*—Mr. J. B. FIFE exhibited to the New Castle and Gateshead Pathological Society a melanotic tumour removed from a man aged 51. Loss of vision, and a slight projection of the eyeball, were the first symptoms remarked by him twenty-five years ago, when he became a patient of Sir J. Fife, who cured him by the use of iodine. The eyeball since that time had been very prominent at two other periods, but was each time replaced by the same remedies. Two years ago it again began to project from the orbit, and notwithstanding all efforts to reduce the tumour that was evidently forcing it forwards, the eye continued to advance, and at the time of the operation was projected beyond the eyelids by an elastic fungoid-like tumour (covered by the thickened and congested conjunctiva), which encircled the eyeball in such a manner that the cornea was alone visible. Until within a few months preceding the operation, no pain accompanied the progress of the disease.

The whole of the contents of the orbit were removed without difficulty. The hemorrhage was very profuse, but quickly ceased on the plugging of the cavity. The tumour entirely filled the orbit, and on being cut open was found as black as coal; it was invested with a covering of cellular tissue, which also ran into the interior of it, separating the whole into lobules: in structure it was soft, and consisted of a cellular tissue, infiltrated with the pigment matter, which was readily washed out of the tissue when incised. The pigment matter under the microscope exhibited innumerable dark-brown organic granules, existing singly, or aggregated together in compound granular cells of great variety of size and form, together with numerous oil-globules, and round and oval colourless cells, about the size of pus-globules, with faint outlines, containing in their interior small shining granules, and occasionally other smaller cells of similar appearance to themselves; also cells of varying size, with thick, dark-brown walls, only very slightly transparent, and containing in the cell-walls numerous dark granules, and occasionally nuclei. These latter cells appeared as if they were the colourless cells transformed by growth and the acquisition of pigment into the peculiar cell of melanosis. The operation was performed three months ago, and the patient reports himself as being quite well, the wounds having healed.—*Ibid.*

[A sufficient time has not elapsed since the operation to pronounce it a successful one.—I. H.]

## MIDWIFERY.

41. *On Compression of the Skull of the Infant during Birth, and its Effects.*—Dr. OGER WARR read a paper on this subject to the Royal Medical and Chirurgical Society (March 11th, 1851). The immediate object of the author of the paper was to draw attention to the compression of the bones of the skull during parturition, and the influence which this exerted on the after condition of the child, whether physical or mental. The subject had been, he considered, much neglected, and he had therefore been induced to bring it before the Society. After stating that he distinguished the deformity produced by its situation, and the bone or bones more immediately implicated in the abnormal condition, he proceeded to enumerate the immediate and remote consequences of the pressure. These consisted of symptoms of imperfect cerebral develop-

ment, and its consequences, together with a train of dyspeptic symptoms, of which flatulence was the most prominent. He also considered that epileptic convulsions and paralysis might result from the same cause. After referring to the influence which Foville attributed to the strange head-dress of the children in Normandy in the production of insanity, the author threw out the hint that compression exerted on the head during parturition might have a similar effect. With respect to the treatment of the effects of compression, the author recommends that when the child is born asphyxiated, free inspiration should be established as quickly as possible; and to effect this, he advises that the child should be made to cry by hitching it with a single twig, &c.; he states the bones of the head may be seen to expand immediately that free respiration is established. He considered that manipulation of the bones of the head was of no service in these cases, unless the whole head was affected. The paralysis and other results which he had enumerated were to be treated in the usual manner, regard always being had to the immediate and peculiar cause of the affection. In the discussion which ensued, the questions mooted had reference chiefly to one point in the paper. Did the pressure exerted on the head by parturition really have such effects as those described? On one side it was contended that such pressure was injurious in the manner described by the author; and cases were related in point to show that such was the fact; in these cases, convulsions and other disorders of the nervous system were present. The experience of Foville also was alluded to with regard to the compression which was exerted by the Norman cap on the heads of children. Two fingers could be put into the indentation so produced. Foville had regarded this as a frequent cause of mania. It was asked whether the pressure on the head of the Child had any influence in the production of mental disease? On the opposite side, however, cases were referred to, in which deformities existed similar to those described in the paper, but the children so affected had no sign or symptoms of disease about them, and the distorted parts became quite restored without interference. In support of the opposition to Dr. Ward's conclusion, the condition of the child's head after delivery was referred to. Frequently after difficult or protracted labours, the heads were of all shapes, and yet how quickly, by the efforts of Nature, did the heads assume their proper form! Even large extravasations of blood were removed in an incredibly short space of time.—*Lancet*, March 15th, 1851.

42. *Fatal Case of Inversion of the Uterus, with Attached Placenta, occurring under the management of a midwife immediately after delivery.*—Dr. E. SMITH related to the Medical Society of London, May 17th, 1851, a case of this accident. "The subject of it was thirty-five years of age, of somewhat large build, and of full habit, and was apparently more sensitive than females are in general. She enjoyed a fair share of health, but was always disposed to complain, and her scrofulous temperament convinced the lack of a robust system. She was ill fitted to resist the influence of exhausting agents. She was delivered of her second child at eleven P.M. on May 5, after a labour of less than the average suffering, and of only five hours' duration. I was called to see her at a quarter past eleven P.M., and found a tumour of about ten inches in diameter, projecting from the external parts. The placenta was chiefly attached to the anterior part, but prolongations of it surrounded the whole tumour in such a manner, that a central cup-shaped concavity existed, of two inches in diameter, and three-quarters of an inch in perpendicular depth. On examination, this tumour proved to be the inverted uterus, to which the placenta was still perfectly adherent. The funis was broken into two parts; that which was attached to the placenta being about eight inches in length, and the portion which was separated being about twelve inches in length. There did not appear to be more hemorrhage than is usually observed when the placenta is detached and expelled in the ordinary mode. She was quite conscious, and fully alive to all that was transpiring, and was not apparently aware of the existence of imminent danger. Yet her pulse could only be felt at intervals, and then only during two or three beats, which were not remarkably frequent. I instantly sent messengers to request the assistance of two neighbouring practitioners, and in the mean time endeavoured to return the uterus, with the placenta still attached,

within the parts, as a preparatory step to the reduction of the inversion. The effort was ineffectual, and the anterior surface of the placenta becoming ruptured at one point, a considerable gush of blood followed. My medical friends arrived in over five minutes after I had first seen the case, and we determined to peel off the placenta first, and then return the uterus. The first was effected by Mr. Lucas, without any further loss of blood, and the latter was also accomplished without much difficulty. The pulse was still almost imperceptible, but the mind was collected and calm. No noises were complained of, nor were there the slightest indications of faintness or spasm. Pure brandy was administered freely, in about half-ounce doses, so frequently, that half a pint was exhibited in three hours, yet no improvement of any but the most temporary character was effected in the circulation. A drowsiness began to steal over the senses in about an hour after the womb was replaced, which the patient attributed, erroneously perhaps, to the effects of the brandy; but from this she could be aroused with more and more effort as the progress of the case advanced. At about one P.M., a sharper state of the countenance became evident, and this increased so much, yet without any complaint whatever, that at from two to three P.M. she had lost her own distinctive features. I frequently introduced my fingers within the patulous os uteri, and placed my hand upon the abdomen, and observed that the uterus remained tolerably contracted, and that at no time was there the slightest hemorrhage. Finding that no benefit whatever had resulted from the administration of the brandy, I again requested the advice of Mr. Lucas at half-past two A.M., when it was determined to move the patient very carefully, but to the extent of relieving the discomfort of the position, and to administer ether. This was done, and the pulse seemed to gain in firmness and steadiness for about a quarter of an hour; but the dullness of the senses still continued. At the end of that period she suddenly appeared to become more exhausted, yet remaining somewhat conscious, and whilst endeavouring to swallow a small quantity of brandy, deglutition failed, and she sank gradually in a few minutes.

The post-mortem examination showed a healthy condition of all the organs of the chest and abdomen, but all were drained of blood. The heart and great vessels were empty. The uterus was properly replaced and empty, having no clots of blood whatever, but some slight remains of the placenta; the cavity was about five inches in the transverse, and eight inches in the longitudinal diameter; the tissue of the uterus was loose and flabby, and the walls at the fundus were fully half an inch in thickness. At the inquest I gave as my opinion of the cause of death, that the patient had died from the shock upon the nervous system, induced partly by the loss of blood and partly by the violent inversion of the uterus.

The points of interest in this case appear to me to be the following: 1st. A confirmation of the ordinary position of the placenta. 2dly. The fact that a bloodless condition of the whole system was induced by an amount of hemorrhage which did not appear to any person present to be considerable, and without any other symptom than pulselessness. The drowsiness which was eventually induced might at least, in some degree, be attributed to the alcohol. At any rate, this and the sharpened features more readily pointed to the nervous than to the circulating systems. 3dly. Considering that the symptoms were rather those indicating a shock than exhaustion from hemorrhage, would it have been prudent to have so far suspended the administration of the brandy as to administer ammonia, ether, or other nervous stimulants? 4thly. Dr. Denman and Dr. Burns advise that the uterus, with the attached placenta, be returned. Dr. Merriman also gives the like counsel, but he mentions an instance in which he first detached the placenta, and the patient did well. It is highly important that some definite rule should be laid down by the profession; for if it be judged best to detach the placenta, it will be injurious to lose time in endeavouring to return it; if it be deemed proper to return the mass, it cannot be right to run the risk of alarming hemorrhage by first detaching the placenta. 5thly. What direct evidence can be adduced to show the guilty interference on the part of the midwife? Cases are recorded of inversion of the uterus having occurred from the mere dead weight of the attached placenta; also from distension of the alimentary canal, or of some sudden action of the abdominal muscles, and there-

fore blame does not, of necessity, attach to the attendant. It is true that, until the placenta is detached, the hand ought not to be for a long time apart from the womb, and in cases where inversion has thus occurred, it has not been so complete as in this case; but yet the admission of the possibility of such an occurrence renders it imperative that direct evidence of a guilty interference or neglect should be established.

The evidences in the case are—1. The admission of the midwife that she attempted to remove the placenta within a quarter of an hour after delivery. 2. A further admission that she did use some force, but that it was to a very slight extent. 3. The rupture of the funis. 4. The persistence of the uterine contraction after the uterus had been returned, and when that organ was almost bloodless and the system exhausted, is *primâ facie* evidence that the inversion had not resulted from the absence of contraction. Still the link was wanting to show that the midwife had used improper force, and thus she escaped punishment. 5. Was transfusion indicated in this case?—*Lancet*, May 24, 1851.

43. On *Diarrhœa* as a hitherto unnoticed symptom of Menstruation, and on the use of Purgatives at the different Epochs of the Menstrual Function.—Dr. TILT, in a paper read before the Medical Society of London (Feb. 22, 1851), premised that, not having found any description of catamenial diarrhœa as a symptom of menstruation, in the classic work of Friend and Brierre de Boismont, he concluded that it was not generally known to the profession, except as a morbid complication of the function. He then proceeded to inquire into the nature of that catamenial diarrhœa, whether it occurs—

1. At the prodroma of menstruation.
2. During its regular establishment.
3. At its cessation.

1. As a symptom of the prodroma of menstruation, diarrhœa scarcely ever occurs. It was noticed but once in 161 cases.

2. As a symptom of regularly-established menstruation, it occurred in 88 instances out of the 161 who were carefully interrogated relative to this point. It did not occur in 72 cases; and in those in which it did occur, it preceded the menstrual flow in 45 cases, it accompanied it in 31, it both preceded and accompanied it in 10, and in two instances it neither preceded nor accompanied, but habitually followed, the menstrual flow for two days; and where there was precursory diarrhœa, the bowels were afterwards in general *costive* until the cessation of the catamenia.

3. As a symptom of menstruation at its cessation, diarrhœa occurs much less frequently than is generally supposed, for it was only found in eight per cent. of such cases. With respect to the nature of the diarrhœa, it is generally unattended by pain, but sometimes nausea and slight colics precede it for two or three days. In one patient, Dr. Tilt found these symptoms continue for eight days previous to the appearance of diarrhœa. When it occurs at the change of life, it generally appears at irregular intervals, though it may adopt the regularity of the menstrual function. As a general rule, however, Dr. Tilt is of opinion that, when diarrhœa has habitually accompanied menstruation, there is, at the change of life, a gradual diminution of both discharges, the cessation of one marking the termination of the other.

From the preceding facts, Dr. Tilt deduced the physiological inference, that, for the performance of the function of menstruation, the ovaries not only determine the menstrual secretion from the womb, but also often call into co-ordinated action most of the organs, which, being subsidiary to nutrition, are animated by the same ganglionic nervous system, and particularly the intestines, with which they are placed in such close juxtaposition. He then laid down the rules which he considered to be those which should be adopted in the administration of purgatives:—

1. During the prodroma.
2. During the regular establishment.
3. At the cessation of menstruation.

1. Use of purgatives during the prodroma of this function.

Dr. Tilt is of opinion that purgatives should not be given at this period, be-

cause they were neither sanctioned by experience nor confirmed by his inquiries.

## 2. Use of purgatives during fully-established menstruation.

As Nature herself has often prefaced the menstrual crisis by a premonitory diarrhoea, so experience teaches that purgatives may be advantageously employed when defective menstruation does not depend on any serious organic lesion. The great point, Dr. Tilt said, in the administration of purgatives, is not to interfere with the menstrual type; for, if this may be done with impunity in a very small class of women, it cannot be so in the majority. Brisk purgatives, given a few days before the symptoms which precede menstruation, often anticipate it by a few days, and thus vex Nature in one of her most constant laws, often producing permanent disorders of that function. An inquiry into how many days before the menstrual flow the premonitory diarrhoea used to appear, or, if the patient has not that symptom, the date of appearance of other menstrual symptoms, will be a sure guide as to the fit time for giving purgatives.

Dr. Tilt next touched on the value of the purgatives in amenorrhoea and in chlorosis, relating that a friend of Morgagni never gave anything, in such cases, but small doses of aloes; and that Dr. Hamilton, of Edinburgh, depended exclusively on purgatives for the cure of chlorosis. Dr. Tilt, however, thought it best not to confide in purgatives alone, but to let them form the initial part of the treatment, as in Nature diarrhoea often forms the initial part of menstruation; and therefore to begin with a decided shock on the system of nutrition by an emeto-cathartic, followed by steel and bitters; but if he finds that the appetite does not improve, and that the bowels remain sluggish, he puts aside the steel and bitters, and seeks to break in on a perverse concentration of forces by giving another emeto-cathartic.

At the period so appositely called by women the dodging time, it is injudicious to give purgatives just before the menstrual epoch; for, says Dr. Tilt, they might increase the flow Nature seeks to diminish; therefore it is more prudent to prescribe the frequent use of the milder opening medicines, so as to diminish, by degrees, the plethora of the abdominal viscera.

## 3. Use of purgatives after the cessation of menstruation.

At this period of life purgatives must be given habitually, for the intention is, not to re-establish periodical discharge, but to diminish plethora, and the necessity for that plethora seeking for any other less manageable seat. As regards what purgative should be given, it is as well, in general, to prescribe the medicine best tolerated by the patient. The soap-and-aloes pill of the Pharmacopœia, ordering five or ten grains to be taken at dinner, Dr. Tilt frequently recommends. He has never seen hemorrhoidal affections caused by this frequent use of nucs, but often relieved by it; and his experience on this point he has found confirmed by that of Avicenna, Stahl, Giacomini, and Cullen. The flour of sulphur alone, or else to each drachm of it a drachm of sesquicarbonate or biborate of soda, and sometimes from five to ten grains of ipecacuanha, may be given in quantities of from one to three scruples of these powders, to be taken once a day, in milk, so as to act mildly on the bowels, which is one of the chief things required at the cessation of menstruation.

Several fellows took part in the discussion on Dr. Tilt's paper. Reference was made to a paper by Dr. Butler Linn, to show that the views advanced by Dr. Tilt had been somewhat anticipated by that gentleman. However, it was admitted that he had rendered a service to science, by the facts which he had advanced in his paper. The debate referred chiefly to two points—the physiological and the pathological ones, to which the paper referred. Various opinions were expressed as to the mode in which diarrhoea might be supposed to act in retarding, interfering with, or promoting, the menstrual discharge. Some difference of opinion existed as to the indications presented for treatment. In his reply, Dr. Tilt stated that he had not seen the essay of Dr. Butler Linn.—*Lancet*, March 15th, 1851.

## MEDICAL JURISPRUDENCE AND TOXICOLOGY.

44. *Case of Rapid Delivery.* By B. W. M'CREADY, M. D., Professor of Materia Medica and Pharmacy in the New York College of Pharmacy. (Communicated to Dr. T. R. BECK.)—I was called in haste about three o'clock in the morning of May 6th, to see Mrs. M'E. I found her supported by a friend, in a sitting posture, over a pot de chambré. She had been taken in labour about an hour previously, had risen to evacuate the bowels, and while sitting on the vessel the child had been born. Believing it to be still connected with her body, she had been afraid to rise, and had remained in her uneasy position till my arrival. Finding that the placenta had already come away, I immediately lifted the patient into the bed, and, removing the child, the whole of the head and part of the body of which were immersed in urine, after dividing the cord, and drying the child's face, I commenced efforts for its resuscitation. In a few minutes I was rewarded by observing a faint effort at respiration, and in half an hour the child cried vigorously.

According to the account of the mother, the child was born immediately after the husband had left the house to request my attendance. As he had to walk nearly a quarter of a mile to my residence, and to return the same distance, we may safely conclude, including the time I occupied in dressing, that he was absent at least fifteen minutes. During all this time the child's head had been under water.

It is to be noticed too that, though it was the patient's third child, she was quite unaware that the placenta had come away, and that neither she nor either of the attendants had presence of mind enough to remove the child from its dangerous position.

45. *Death from the External Application of Arsenic.* By B. W. M'CREADY, M. D., Prof. of Mat. Med. &c. in the New York College of Pharmacy. (Communicated to Dr. T. R. BECK.)—In the latter part of August, 1844, I was called to the family of a poor woman, residing in Walker St. I found two children, about five and seven years of age, with their faces swollen and oedematous, particularly about the eyes, and the skin red and shining. The scalp was covered with a copious eruption of porrigo favosa, and careful examination discovered upon it, adhering in points to the pustules and about the roots of the hair, some reddish ointment. On questioning the woman, she stated that, on the day previous, she had applied at the dispensary to have her children treated for the eruption, and had been furnished with the ointment in question (ung. hydrarg. oxyd. rubr.). This had been rubbed thoroughly into the affected parts, and had been followed by the swelling and redness of the face. The next morning the children appeared much better. She now informed me that, following the advice of an old woman, she had obtained sixpence worth of arsenic (arsen. alb.), had mixed it with a gill of gin, and rubbed it well into the children's heads just previous to the application of the ointment. At the same time a younger child (about two years of age), likewise affected with P. favosa, was shown me, when a similar course had at the same time been pursued. This child's countenance was pale, and there was no sign of inflammation about the scalp, but it was suffering from diarrhoea and tenesmus, with great weakness of the lower extremities. The same night it died, its lower extremities having first become completely paralyzed. At the coroner's inquest, the old woman, who had advised the treatment, testified that she had repeatedly used the same means in scald head, and always without bad consequences, and with the cure of the eruption. The mother stated that she had on one previous occasion applied the arsenic, and though the application was followed by some swelling, this soon subsided, and the head seemed much better. The quantity of arsenic obtained must have been about half an ounce, and this, having been previously added to the gin, and the bottle well shaken, had been rubbed freely on the scalps of the three children. It will be noticed that, in the child that died, the application was followed by no marks of local inflammation. No post-mortem examination was made. The other two children speedily recovered.

46. *Medical Evidence, in favour and at the request of the Prisoner.*—(See *Beale's Med. Jurisprudence*, edition of 1850, vol. ii. p. 911.)

Central Criminal Court, London, Aug. 22, 1848. *Regina v. Spry & Dore.*

The coroner's jury having returned a verdict of wilful murder against the prisoners, Clarkson applied on their behalf, previous to the trial, that Dr. Taylor might be permitted to inspect the stomach of the deceased and its contents, which were at present in the keeping of a police inspector. He considered it absolutely essential to the interests of the accused that this should be done, in order that they might be prepared at the trial with evidence, which nothing but a minute examination could furnish.

THE RECORDER. This is a very unusual application, and one that I am not quite sure the court has jurisdiction to entertain; but as it appears to be made in furtherance of the interests of justice, I think I may take upon myself to make the order. The examination, however, should take place in the presence of the officer who has custody of the stomach, as well as in that of the medical gentleman who examined it on the part of the prosecution. The expense must be borne by the prisoners, and I think the coroner should have notice of the time and place of the examination. I have the less difficulty in granting the application, because the judge would no doubt, at the trial, stop the case until such examination had been made.—*Cox's Reports of Cases in Criminal Law*, vol. iii. p. 221.

T. R. B.

47. *Wounding.*—The following are cases apparently parallel, yet not without anatomical reasons to justify the legal decisions:—

Central Criminal Court, January, 1848. Before Mr. Justice Patteson. *Reg. v. Jones.*—The prisoner was indicted for wounding with intent to do grievous bodily harm. Bedkin, for the prosecution, stated the nature of the wound to be as follows: The prisoner had come behind the prosecutrix, and given her a violent kick in the private parts, and that had been followed by an occasional discharge of blood, mingled with urine; but the surgeon could not undertake to say from what precise vessels the blood originally flowed.

PATTESON, J. Then I do not think the more serious charge in this indictment sustainable. There may have been no lesion of any of the vessels at all. Blood may be discharged from those parts simply from natural causes.

*Ibid.* Feb. 3, 1849. Before Mr. Justice Creswell. *Reg. v. Walltham.*—The prisoner was indicted for wounding the prosecutor, with intent to do him grievous bodily harm. It appeared from the evidence that the prosecutor, who was a policeman, while endeavouring to separate the prisoner and a man with whom he was fighting, received from the former a violent kick in his private parts. From the testimony of the surgeon, it appeared that the external skin was unbroken, but that the lining membrane of the urethra was ruptured, which caused a small flow of blood mingled with urine, for two days. That membrane is precisely the same in character as that which lines the cheek and the external and internal skin of the lip.

Perry (for the prisoner) objected that this was not a wounding within the statute, and cited *Regina v. Jones*; that case was very similar to the present; there is no external wound.

CRESWELL, J. If the cases were similar, I should abide by the ruling of Mr. Justice Patteson, but there is a great difference between them. I think this is a wounding within the statute.—*Cox's Criminal Law Cases*, vol. iii. pp. 441, 442.

T. R. B.

48. *Rape.*—The *Queen v. Ryan.* The prisoner was indicted for rape. The prosecutrix was an idiot, and when asked questions in the witnesses' box, was evidently unconscious of their import, and not in a condition to understand right from wrong. Platt, B. interrogated her father as to her general habits, whether they were those of decency and propriety, and an answer in the affirmative was returned.

PLATT, B. in summing up: The question is, did the connection take place with her consent? It seems that she was in a condition incapable of judging, and it is important to consider whether a young person, in such a state of incapacity,



was likely to consent to the embraces of this man; because if her habits, however irresponsible she might be, were loose and indecent, there might be a probability of such consent being given, and a jury might not think it safe to conclude that she was not a willing party. But here the presumption is that the young woman would not have consented, and if she was in a state of unconsciousness at the time the connection took place, whether it was produced by any act of the prisoner, or by any act of her own, any one having connection would be guilty of a rape. If you believe that she was in a state of unconsciousness, the law assumes that the connection took place without her consent, and the prisoner is guilty of the crime charged. The prisoner was convicted.—*Cox's Criminal Law Cases*, vol. ii. p. 115.

T. R. B.

49. *Rape*.—*Rex v. Camplin*. The details of this case will be found in *Beck's Medical Jurisprudence*, last edition, vol. i. p. 183. I find the following note in the first volume of *Denison's Crown Cases Reversed*, which seems important to the full consideration of this trial:—

"Mr. Baron Parke has kindly furnished the editor with the following note of the reasons for the opinion of the judges in the case of *Rex v. Camplin*:—

"Of the judges who were in favour of the conviction, several thought that the crime of rape is committed by violating a woman when she is in a state of insensibility, and has no power over her will, whether such state is caused by the man or not, the accused knowing at the time that she is in that state, and Tindal, C. J., and Parke, B., remarked that, in *Statute of Westminster, 2, C. 34*, the offence of rape is described to be ravishing a woman 'where she did not consent,' and not ravishing against her will. But all the ten judges agreed that in this case, where the prosecutrix was made insensible by the act of the prisoner, and that an unlawful act, and when also the prisoner must have known that the act was against her consent at the last moment that she was capable of exercising her will, because he had attempted to procure her consent and failed, the offence of rape was committed."

The other three judges did not think that this could be considered as being sufficiently proved.

T. R. B.

50. *Strangulation in Utero*.—Dr. FRENEY, of Remenville, communicates the following:—

On the 9th of October, I was called to attend a female, aged forty-three, in labour. She was small, but well made, and the mother of five children. The pains were regular and strong, the head presented and was low down, and in a quarter of an hour the child was born. It was motionless, the flesh soft and the lips red. The umbilical cord passed twice round the neck, and then under the left thigh, in such a manner that with every motion in the womb of the mother, the traction was immediately increased on the neck. On removing the circumvolutions of the cord, Dr. Freney was surprised to find a depression in the neck, corresponding with the size of the cord, and this was paler than the surrounding skin. After fifteen minutes of incessant efforts, respiration appeared, and the child is now living.

Dr. Freney inquires what would have been the opinion concerning these appearances in a case supposed to be criminal, particularly as he was obliged to use the insufflation of air into the lungs.

The answer of the editor is quite satisfactory. The present is a remarkable and an uncommon case. It illustrates the possibility of the umbilical cord acting as a cause of strangulation in the womb of the mother. Still the circular impression observed is only the semblance of what would have resulted from the actual application of a cord. There was no ecchymosis; the furrow was neither deep nor unequal, as is seen in ordinary strangulation. It is impossible, as the child survived, to show that the furrow did not in this instance take the appearance of parchment; or that the subjacent cellular tissue was injured. Each of these is seen in instances of criminality.—*Journal de Médecine et de Chirurgie*, Nov. 1850.

T. R. B.

51. *Two Cases of Poisoning by Arsenic, and Remarkable Recovery under the free Use of Tartarized Antimony.*—The subjoined cases occurred recently in the practice of Mr. BEARDSLEY, of Hleanor, formerly n student n Guy's Hospital. They illustrate forcibly the propriety of producing an early expulsion of the poison from the stomach by the aid of n quickly neting emetic.

*Case 1.*—The first case was that of n little hoy, six years old, who swallowed a teaspoonful of the white arsenic of the shops. It was left on the floor of the house during the night, for the crickets to eat, and the child getting up first in the morning, found it, and eat it all up. In a very few minutes he was taken ill: there was vomiting; pain at the stomach; excessive thirst; cold extremities; quick and feeble pulse. At this juncture the parents had some vin. ant. tart. in the house, which they administered, and the child appeared better. When Mr. Beardsley arrived and was informed of this, he gave a full dose of the antim. tart.; vomiting was freely excited; the bowels were rapidly opened, and the child recovered with no further had symptoms. The child was not seen to eat the arsenic, and possibly some might have been epilled, but none could be found, and the child was followed down stairs as quickly as possible; but the poison had been swallowed.

*Case 2.*—The next caso was that of n girl twenty-two years of age. She bought two drachms of white arsenic, with the intention of poisoning herself. She was observed all Monday to be very unsettled, and refused food at dinner. Just before tea she purchased the arsenic, and, instead of tea, she put the arsenic into a cup, and poured hot water over it, and drank it off. She was immediately seized with violent pain in the region of the stomach; burning heat in the throat, and syncope. Mr. Beardsley was immediately sent for. When he saw her, which was only *half an hour* after she had taken the arsenic, she had cold extremities; very anxious look; pulse quick and hurried; violent pain in the stomach and bowels, which was very much aggravated on pressure.

The fauces had been tickled to excite vomiting, but ineffectually; and an ounce of castor oil and some milk had been administered. An emetic of sulphate of zinc was now given to her, but as this did not act, six grains of emetic tartar were subsequently administered. Most copious vomiting ensued, and the patient immediately said she was better. She stated that the ejected matter was similar in taste to that which she had taken in the cup, and that it was *sweetish*. Warm water and more emetic tartar (three grains) were administered, and she again vomited two or three times. She took two more doses of tartar emetic, and then violent purging, as well as vomiting, ensued. Next morning she was better; the pain had entirely left her. She took the remainder of the antimonial mixture, and she perfectly recovered; in fact she was almost well on Wednesday; she only felt debilitated on the Tuesday.

In this case, *two drachms* of powdered arsenic were taken in *warm water*, on an *empty stomach*. THE TREATMENT WAS WHAT DR. BECK SAYS SHOULD NEVER BE ADOPTED, AND YET THE RECOVERY WAS RAPID IN BOTH CASES. It may be mentioned that the girl had some melted butter given to her, before vomiting was established, i. e. before the emetic tartar was administered.

*Remarks.*—The use of tartarized antimony as an emetic has been generally condemned in cases of poisoning, because it has been supposed to have a tendency, during the nauseating stage, to promote the absorption of a poison. The above cases, and some others which might be quoted, show that this danger is of n hypothetical kind, and based on physiological principles not applicable to the treatment of such cases. If tartarized antimony aids absorption, it very quickly promotes the expulsion of the poison by vomiting; and the latter mode of operation is much more beneficial than the former is injurious. Better evidence of this could not be afforded than that yielded by the two cases here reported. Case 1 was that of n child only six years old. The quantity of poison swallowed was a teaspoonful, about 150 grains, an enormous dose when we consider the age of the patient. Further, it was swallowed in the early part of the morning, while the stomach was empty; and the rapid action of the poison was shown by the very early occurrence of the symptoms. Admitting that the whole teaspoonful of arsenic was not taken, it is quite clear that enough was swallowed to cause death, and the good effect of the tartar emetic was well indicated by its removing the early collapse.

—I have taken the above narrative from a number of the *London Medical Examiner*, which Dr. Hays was kind enough to send me. Subsequently, I have found that it is correctly copied from the *Guy's Hospital Reports*, vol. vii. part 1, p. 203.

I regret extremely that Mr. Taylor should have taken occasion to represent these trifling (I use this term considerably) cases in an offensive manner as respects myself. In everything I have written on the subject of Medical Jurisprudence, I have endeavoured to do him and have done him ample justice. Due credit has been given, whenever anything original with him could be discovered. I challenge on this point the most careful scrutiny, commencing with his work entitled "*ELEMENTS OF MEDICAL JURISPRUDENCE*," by Alfred S. Taylor, vol. i. 8vo., London, 1836."

In the first edition of my work entitled "*Elements of Medical Jurisprudence*," published in 1823 (vol. ii. p. 254), when considering the proper treatment in cases of poisoning by arsenic, I said "*Tartrate of antimony should never be given*." I continue of the same opinion still, and accordingly made the same observation in the 10th edition, published in Dec. 1850, vol. ii. p. 554: "*Tartrate of antimony should never be given*." I warn every AMERICAN physician who reads this, that the recovery of a person in such a case is merely an escape, and that, although two may have recovered (from fortunate circumstances, such indeed as scarcely ever occur in instances of poisoning by arsenic), yet the third case, in which pure tartrate of antimony is administered, will probably prove fatal. But this, after all, is not the matter brought before the medical public by Mr. Taylor.

"The treatment" (he remarks) "was what Dr. Beck says should never be adopted, and yet the recovery was rapid in both cases." If Mr. Taylor had been actuated by the proper feelings, he would have written a far different sentence. He would have said, "The treatment was what Dr. Beck says should never be adopted," (and in which statement he was perfectly justified by at least the negative testimony of Foderé, Orfila, Christison and others, and in particular by my own writings,) "and yet the recovery was rapid in both cases." I do not add Paris and Fonhlaque to the above authorities, for reasons which I shall presently assign.

The unjust and uncalled for remark of Mr. Taylor has induced me to look over the authorities consulted in preparing the first edition of my book. Foderé advises the use of warm water to promote vomiting, and, if this fail, then the use of the stomach-pump. Gordon Smith says that some do not hesitate to employ the most active emetics, "considering that the irritation of much fluid is an advantage in these cases. Others advocate bland liquids, as warm water, oil, linseed or chamomile tea." Orfila (*Séours à donner aux Persons empoisonnées*) advises that vomiting should be promoted with many tumblerfuls of sugared water, or of warm or cold water, decoction of marshmallow root, or flaxseed tea. A mixture may also be given of equal parts of lime-water and sugared water.

What says Christison in his work on poisons—a work of some considerable authority in this country, although he may be elsewhere supplanted by later authorities? "If vomiting should be delayed, as often happens, for half an hour or more, advantage ought to be taken of the opportunity to administer an emetic of the sulphate of zinc, with the view of withdrawing the powder in mass before it is diffused over the stomach, and for the same purpose, milk should be drunk both before and after vomiting has begun, as it appears to be the best substance for enveloping the powder, and so procuring its discharge. The patient should never be allowed to exhaust his strength in retching, without a little milk or other fluid in his stomach."

And again, "Even emetics are unnecessary, when vomiting has commenced in consequence of the operation of the poison. When milk in sufficient quantity cannot be procured, strong farinaceous decoctions will probably prove useful," 2d edition, p. 322. The same is repeated in the 3d edition, p. 335, and again in the 4th edition, p. 365.

Is there anything in all the above, advising or recommending, or justifying, or even allowing, the use of tartrate of antimony?

But what says Mr. Taylor, the author of a work on Medical Jurisprudence, and a Treatise on Poisons, concerning this matter? In the first edition of "*A Manual of Medical Jurisprudence, by Alfred S. Taylor, &c., &c.*," London, 1844, under the head of "TREATMENT" in cases of poisoning by arsenic (p. 143), the following remarks appear: "If vomiting does not already exist as a direct effect of the poison, sulphate of zinc may be exhibited, and the emetic effects promoted by mucilaginous drinks, such as linseed tea. When sulphate of zinc cannot be procured, a good substance for an emetic is powdered mustard, in the proportion of from one to two teaspoonfuls, in a glass of water, administered at intervals. A saponaceous liquid, made of equal parts of oil and lime-water, may also be given. While this invests the poison, the lime acts in some degree as a chemical antidote; although as arsenic is usually taken in the form of a coarse powder, and is very little soluble, chemical antidotes are not of much use. The stomach-pump may be usefully employed; but, unless the patient is seen early, remedial means are seldom attended with success." I pause to inquire whether tartrate of antimony is enumerated among the remedial means of Mr. Taylor.

In the 2d edition of the same *Manual*, London, 1846, p. 131, the same observations as above are repeated in the same words. In the 3d edition of the same *Manual*, London, 1849, p. 73, the same are again repeated; but the following remarks are added: "Mr. Tubbs informs me that, conjoined with the use of the stomach-pump and emetics of sulphate of zinc, he has found great service in a mixture of milk, lime-water, and albumen. Such a mixture is undoubtedly well fitted to envelop the particles of arsenic, and sheathe the coats of the stomach from the irritant action of the poison. This gentleman has sent me the report of no less than nine cases, some of them of a very severe kind, which he has thus successfully treated."

But besides the three editions of his *Manual*, Mr. Taylor has also published a work "*On Poisons, in relation to Medical Jurisprudence and Medicine.*" The copy I own was published in 1848, and is of as large bulk as the third edition of the *Manual*. It is, therefore, just to suppose that it will be more ample, on the subject of poisons, than it would have been possible to notice in the various editions of the *Manual*. Of course, it also considers the "Treatment in cases of poisoning by arsenic." And, at the risk of proving tedious, I will quote all that Mr. Taylor says respecting vomiting. If nothing else, it will prove how easy it is to pour *solid* as well as *liquid* substances from one receptacle into another.

"If vomiting does not already exist as a direct effect of the poison, sulphate of zinc should be exhibited, and its emetic effects promoted by mucilaginous drinks, such as linseed tea, milk, or albuminous liquids. When sulphate of zinc cannot be procured, a good substance for an emetic is powdered mustard, in the proportion of from one to two teaspoonfuls in a glass of water, administered at intervals. A saponaceous liquid, made of equal parts of oil and lime-water, may also be given. While this invests the poison, the lime tends to render less soluble that portion of the poison which is dissolved. The stomach-pump may be usefully employed; but, unless the patient is seen early, remedial means are seldom attended with success. I have known death to occur in a case where every particle of poison was found, on subsequent examination, to have been removed from the stomach. There are many instances on record in which the arsenic appears to have been early ejected by constant vomiting and purging. The recovery has, however, been commonly attributed to the supposed antidote. Mr. Tubbs, however, informs me that, conjoined with the use of the stomach-pump, and emetics of sulphate of zinc, he has found great service in a mixture of milk, lime-water, and albumen. Such a mixture is undoubtedly well fitted to envelop the particles of arsenic, and sheathe the coats of the stomach from the irritant action of the poison."—(p. 333.)

Such were Mr. Taylor's rules of practice, in addition to those that I have previously quoted. But I have not quite done with his "*Treatise on Poisons.*" He furnishes us also with certain *general* remarks on the "Treatment in Cases of Poisoning," and, at page 79, are the following:—

"*Emetics.*—With regard to clearing the stomach, the irritant poisons in

general act as powerful emetics, and therefore all that we have to do is to promote vomiting by the free administration of warm water. In the case of oxalic acid, however, it is desirable to avoid giving much water, as the poison is dissolved and rapidly diffused over a large surface of the intestines, whereby it becomes more easily absorbed. If the vomiting should cease, or if it should not have taken place as an effect of the poison, we must then give an emetic of sulphate of zinc or copper, i. e. from fifteen to twenty grains at a dose, and, unless attended with marked effect, this must be repeated every quarter of an hour. IPECACUANHA AND TARTAR EMETIC SHOULD BE AVOIDEN, for they are liable to produce excessive nausea, and during this state the poison becomes more rapidly absorbed. Supposing that we have not at hand the sulphate of zinc or copper, there is scarcely a house in the country that does not furnish an emetic in the shape of common mustard. From a teaspoonful to a dessertspoonful of this, in half a glass of warm water, may be given to the patient every five or ten minutes, according to the effect. The back of the throat may be at the same time tickled with a feather. If there should be no mustard, then a thick mixture of yellow soap and warm water may serve as a substitute."

"It is right to bear in mind that most of these emetic substances are themselves irritant, and therefore a proper degree of caution must be observed in giving them. In all cases, where the symptoms are those of irritant poisoning, the administration of viscid or mucilaginous liquids is advisable. Barley-water, decoction of linseed, flour and water mixed in the form of paste, albumen, or a mixture of oil and lime-water, or yellow soap and water, may be freely given. These viscid substances serve to protect the coats of the stomach, to lock up the undissolved particles of poison, and therefore favour its expulsion during the act of vomiting."

The reader has now before him the varying opinions of Mr. Taylor on the propriety of the exhibition of tartar emetic in cases of poisoning by arsenic, and he will judge, accordingly, of the propriety of bringing forward my name so prominently.

I have, in another place, stated that I would not quote Paris and Fonblanque, and my reason I may now briefly give. Dr. Paris received a certain prize, as the author of the best work on medical jurisprudence, much to the annoyance of Mr. Taylor. I am content with the decision, particularly if the statement of the *London Medical Examiner* be correct, that Mr. Taylor is the editor of the *London Medical Gazette*. I have been at a loss to understand the contemptible remarks and insinuations occasionally appearing in its pages, until my suspicions were thus resolved into certainty. I doubt, however, whether the matter would have rested to the present time, if my friends Dr. Darwall, and particularly Dr. William Dunlap (the "Tiger Dunlap" of *Fraser's Magazine*) had not gone down to the grave. There is another, but recently deceased, who also keenly felt the anonymous quirps and hints of a pettifogging eritic, and who only waited for a name, to place the individual in his proper light before the medical public.

In all this, let it be distinctly understood that my appeal is to the American Medical Profession alone. I expect no justice from "Household" cliques of reviewers, whose power and influence are dependent on the frail life of a female, and who use their temporary ascendancy in depreciating whatever interferes with their present objects.

T. R. B.

52. *Researches on Gestation.* By M. COSTE.—Of the young females whom I have dissected at the Morgue in Paris, there were quite a number in whom the ovum was either in a state of complete maturation, or detached, and on its way to the uterus. In all of these, I found the uterine lined with a mucous membrane of such thickness that, if its constancy had not proved to me that it was a natural result, I should have deemed it a morbid change.

This membrane, of which I speak, is in a great measure formed by glands opening on the inner surface, by orifices visible to the naked eye. It has a thickness of at least a fourth, and indeed a third, of that of the muscular one, and in some instances it forms circumvolutions or folds pressed upon each other

in the uterine cavity. The anatomical specimens herewith presented will prove this.

In cases of extra-uterine pregnancy, this mucons membrano is still greatly increased. It forms folds as voluminous as the cerebral circumvolutions, and is sometimes not less than ten millimetres in thickness.

It is thus demonstrated that whenever the ovum arrives at maturity in the ovarium, or when it is detached, the uterine mucus undergoes an evolution, to receive it. And this being proved, it remained to ascertain whether the Fallopian tubes were always open throughout their whole length, or whether, at their junction with the uterus, they were closed with a pseudo-morphous membrane, destined to become the deciduous membrane. My observations incline me to the first opinion.

In females dying through suicide from the twentieth to the thirtieth day after conception (as well as in females the condition of whom I have already noticed), the tubes communicate freely with the uterus, and the mucous membrane, having become still more thickened, is also present; but here the ovum, instead of being in the uterine cavity, where I expected to find it, was in fact buried in the coats of the hypertrophied mucons membrane. So manifestly is this shown in the anatomical specimens accompanying the memoir, that, if the uterus alone had been examined, it would have been deemed extremely doubtful whether pregnancy had occurred.

Pregnancy is thus interstitial. The ovum, after passing into the uterus, is probably buried, within a few days, in the mucous membrane of that organ. In a short time, it enlarges and distends its covering—which, in the opinion of M. Coste, finally develops into the three membranes, or folds, which have been noticed by anatomists—the parietal or uterine, the scrotine or placental, and the reflected. These organs are exfoliated after delivery, and, in proof of this, he exhibited specimens taken from females who died in twenty-four hours and in four weeks thereafter.

M. Coste is of opinion that, from the above results, we can explain various phenomena as yet doubtful or undetermined as to their cause, and he enumerates, among these, the hemorrhages that follow delivery, the lochia, and indeed certain cases of sterility. He promises a second memoir.—*Comptes Rendus*, vol. xxxi. No. 17, October 21, 1850.

T. R. B.

53. *Pathological Appearances in Cases of Strangulation or Hanging, Apoplexy, &c.*, (from Professor J. H. BENNER's Clinical Lectures).—*The Pathological Laws which Regulate Diseased Functions of the Nervous System*.—For the purpose of diagnosis and treatment, it is a matter of great importance to attend to the following generalizations:—

(1.) *The amount of fluids within the cranium must always be the same so long as its osseous walls are capable of resisting the pressure of the atmosphere.* There are few principles in medicine of greater practical importance than the one we are about to consider—the more so, as many able practitioners have lately abandoned their former opinions on this head, and on what I consider to be very insufficient grounds. On this point, therefore, I cannot do better than condense and endeavour to put clearly before you the forcible arguments of the late Dr. John Reid, with such other considerations as have occurred to myself.

That the circulation within the cranium is different from that in other parts of the body, was first pointed out by the second Monro. It was tested experimentally by Dr. Kellie of Leith, ably illustrated by Dr. Abercrombie, and successfully defended by Dr. John Reid. The views adopted by these distinguished men were, that the cranium forms a spherical bony case, capable of resisting the atmospheric pressure, the only openings into it being the different foramina by which the vessels, nerves, and spinal cord pass. The encephalon, its membranes and blood-vessels, with perhaps a small portion of the cerebro-spinal fluid, completely fill up the interior of the cranium, so that no substance can be dislodged from it without some equivalent in bulk taking its place. Dr. Monro used to point out that a jar, or any other vessel similar to the cranium, with unyielding walls, if filled with any substance, cannot be emptied without air or some other substance taking its place. To use the illustration of Dr. Watson,

the contents of the cranium are like beer in a barrel, which will not flow out of one opening, unless provision be made at the same time that air rushes in. The same kind of reasoning applies to the spinal canal, which, with the interior of the cranium, may be said to constitute one large cavity, incompressible by the atmospheric air.

Before proceeding further, we must draw a distinction between pressure on, and compression of, an organ. Many bodies are capable of undergoing a great amount of pressure without undergoing any sensible decrease in bulk. By compression must be understood, that a substance occupies less space from the application of external force, as when we squeeze a sponge, or compress a bladder filled with air. Fluids generally are not absolutely incompressible, yet it requires the weight of one atmosphere, or fifteen pounds on the square inch, to produce a diminution equal to  $\frac{1}{25,555}$ th part of the whole. Now this is so exceedingly small a charge upon a mass equal in bulk to the brain, as not to be appreciable to our senses. Besides, the pressure on the internal surface of the blood-vessels never exceeds ten or twelve pounds on the square inch, during the most violent exertion, so that, under no possible circumstances, can the contents of the cranium be diminished even the  $\frac{1}{25,555}$ th part. When the brain is taken out of the cranium, it may, like a sponge, be compressed, by squeezing fluid out of the blood-vessels; but during life, surrounded, as it is, by unyielding walls, this is impossible. For let us, with Abercrombie, say that the whole quantity of blood circulating within the cranium is equal to 10; 5 in the veins, and 5 in the arteries; if one of these be increased to 6, the other must be diminished to 4, so that the same amount, 10, is always preserved. It follows that, when fluids are effused, blood extravasated, or tumours grow, a corresponding amount of fluid must be pressed out, or of brain absorbed, from the physical impossibility of the cranium holding more matter. At the same time, it must be evident that an increased or diminished amount of pressure may be exerted on the brain, proportioned to the power of the heart's contraction, the effect of which will be, not to alter the amount of fluids within the cranium, but to cause, using the words of Abercrombie, "a change of circulation" there.

Dr. Kellie performed numerous experiments on cats and dogs, in order to elucidate this subject. Some of these animals were bled to death by opening the carotid or femoral arteries; others by opening the jugular veins. In some the carotids were first tied, to diminish the quantity of blood sent to the brain, and the jugulars were then opened, with the view of emptying the vessels of the brain to the greatest possible extent; while, in others, the jugulars were first secured, to prevent as much as possible the return of the blood from the brain, and one of the carotids was then opened. He inferred, from the whole inquiry, which was conducted with extreme care, "That we cannot, in fact, lessen, to any considerable extent, the quantity of blood within the cranium by arteriotomy or venesection; and that when, by profuse hemorrhages destructive of life, we do succeed in draining the vessels within the cranium of any sensible portion of red blood, there is commonly found an equivalent to this spoliation in the increased circulation or effusion of serum, serving to maintain the plenitude of the cranium."

Dr. Kellie made other experiments upon the effects of position immediately after death from strangulation or hanging. He also removed a portion of the unyielding walls of the cranium in some animals, by means of a trephine, and then bled them to death; and the differences between the appearances of the brain in these cases, and in those where the cranium was entire, were very great. One of the most remarkable of these differences was its shrunken appearance in those animals in which a portion of the skull was removed, and the air allowed to gravitate upon its inner surface. He says: "The brain was sensibly depressed below the cranium, and a space left, which was found capable of containing a teaspoonful of water."

It results, from these inquiries, that there must always be the same amount of fluids within the cranium so long as it is uninjured. In morbid conditions these fluids may be blood, serum, or pus; but in health, as blood is almost the only fluid present (the cerebro-spinal fluid being very trifling), its quantity can undergo only very slight alterations. There are many circumstances, however,

which occasion local congestions in the brain, and consequently unequal pressure on its structure, in which case another portion of its substance must contain less blood, so that the amount of the whole, as to quantity, is always preserved. These circumstances are mental emotions, hemorrhages, effusions of serum, and morbid growths. Some congestions, or local hyperæmias, in themselves constitute morbid conditions; and nature has, to a great extent, provided against their occurrence, under ordinary circumstances, by the tortuosity of the arteries and the cerebro-spinal fluid, described by Magendie.

The views now detailed had been very extensively admitted into pathology, when Dr. Burrows, of St. Bartholomew's Hospital, endeavoured to controvert them, first in the Lunnleian Lectures of 1843, and subsequently in a work published in 1846, entitled, "On Disorders of the Cerebral Circulation, and on the Connection between Affections of the Brain and Diseases of the Heart." Dr. Burrows, however, evidently formed the most confused notions of the doctrine we are advocating; for, instead of stating it as propounded by its authors, he *actually misrepresented* it, as Dr. Reid pointed out. Thus, he is always combating the idea that blood-letting, position, strangulation, &c., cannot affect the blood in the brain; whereas the real proposition is, that they cannot alter the fluids within the cranium. By thus confounding blood with fluid, and brain with cranium, he has only contrived to overthrow a theory of his own creation.

Dr. Burrows has brought forward several observations and experiments, which he considers opposed to the theory now advocated. His facts are perfectly correct. I myself have repeated his experiments on rabbits, and can confirm his descriptions. It is the inferences he draws from them that are erroneous. For the paleness which results from hemorrhage, and the difference observable in the colour of the brain, when animals, immediately after death, are suspended by their ears or by their heels, is explicable by the diminished number of coloured blood particles in the one case, and by their gravitation downwards in the other. That the amount of fluid within the cranium was in no way affected, is proved by the plump appearance of the brains figured by Dr. Burrows, and the total absence of that shrunken appearance so well described by Dr. Kellie.

Neither does our observation of what occurs in asphyxia or apnoea oppose the doctrine in question, as Dr. Burrows imagines, but rather confirms it. On this point the following observations by Dr. John Reid are valuable. He says: "If any circumstance could produce congestion of the vessels within the cranium, it would be that of death by hanging; for then the vessels going to and coming from the brain are, with the exception of the vertebral arteries, compressed and then obstructed. These two arteries, which are protected by the peculiarity of their course through the foramina of the transverse processes of the cervical vertebrae, must continue for a time to force their blood upon the brain, while a comparatively small quantity only can escape by the veins. Indeed, the greater quantity of blood carried to the encephalon by the vertebrals returns by the internal jugulars, and not by the vertebral veins, which are supplied from the occipital veins of the spinal cord; and the anastomoses, between the cranial and vertebral sinuses, could carry off a small quantity of the blood only, transmitted along such large arteries as the vertebrals. And yet it is well known that there is no congestion of the vessels within the cranium after death by hanging, however gorged the external parts of the head may be by blood and serum." This is admitted by Dr. Burrows, although he endeavours to get rid of so troublesome a fact by a gratuitous hypothesis, which will not bear a moment's examination, but for the refutation of which I must refer to the works of Dr. Reid.\*

On the whole, whether we adopt the expressions of local congestion, of change of circulation within the cranium (Abercrombie), or of unequal pressure (Burrows), our explanation of the *pathological* phenomena may be made equally correct, because each term implies pretty much the same thing. But if we

\* Monthly Journal, August, 1846: Physiological, Anatomical, and Pathological Researches, No. XXV.



imagiae that resection will enable us to diminish the amount of blood in the cerebral vessels, the theory points out that this is impossible, and that the effects of bleeding are explained by the influence produced on the heart, the altered pressure on the brain, exercised by its diminished contractions, and the change of circulation within the cranium thereby occasioned.

I have entered somewhat fully into this theory, because, independent of its vast importance in a practical point of view, it is one which originated in, and has always been maintained by, the Edinbrough School of Medicine. Singular to say, notwithstanding the obvious errors and fallacies in Dr. Burrows' work, as soon as it appeared the whole medical press of England and Ireland adopted its conclusions, and even Dr. Watson, in the last edition of his excellent work, also abandoned the theory of Monro, Kelly, and Abercrombie. But so far is this theory concerning the circulation within the cranium from being shaken by the attack of Dr. Burrows, that it may be said now to stand on a firmer basis than ever, owing to that attack having drawn forth the convincing reasoning and unanswerable arguments of so sound an anatomist, physiologist, and pathologist as the late Dr. John Reid.

(2.) *All the functions of the nervous system may be increased, perverted, or destroyed, according to the degree of stimulus or disease operating on its various parts.* Thus, as a general rule, it may be said that a slight stimulus produces increased or perverted action; whilst the same stimulus, long continued or much augmented, causes loss of function. All the various stimuli, whether mechanical, chemical, electrical, or psychical, produce the same effects, and in different degrees. Circumstances influencing the heart's action, stimulating drinks or food, act in like manner. Thus, if we take the effects of alcoholic drink, for the purpose of illustration, we observe that, as regards combined movements, a slight amount causes increased vigour and activity in the muscular system. As the stimulus augments in intensity, we see irregular movements occasioned, staggering, and inability of directing the limbs. Lastly, when the stimulus is excessive, there is complete inability to move, and the power of doing so is temporarily annihilated. With regard to sensibility and sensation, we observe cephalalgia, tingling, and heat of skin, tinnitus aurium, confusion of vision, muscæ volitantes, double sight, and lastly, complete insensibility and coma. As regards intelligence, we observe at first rapid flow of ideas, then confusion of mind, delirium, and lastly sopor and perfect unconsciousness. In the same manner pressure, mechanical irritation, and the various organic diseases, produce augmented, perverted, or diminished function, according to the intensity of the stimulus applied, or amount of structure destroyed.

Thus it has been shown, that excess or diminution of stimulus, too much or too little blood, very violent or very weak cardiac contractions, and inflammation or extreme exhaustion, will, so far as the nervous functions are concerned, produce similar alterations of motion, sensation, and intelligence. Excessive hemorrhage causes muscular weakness, convulsions, and loss of motor power, perversions of all the sensations, and lastly, unconsciousness from syncope. Hence the general strength of the frame cannot be judged of by the nervous symptoms, although the treatment of these will be altogether different, according as the individual is robust or weak, has a full or small pulse, &c. These similar effects on the nervous centres from apparently such opposite exciting causes, can, it seems to me, only be explained by the peculiarity of the circulation previously noticed. A change of circulation within the cranium takes place, and whether arterial or venous congestion occurs, pressure on the organ is equally the result. The importance of paying attention to this point in the treatment must be obvious.

(3.) *The seat of the disease in the nervous system influences the nature of the phenomena or symptoms produced.* It is a matter of very great importance to ascertain how far certitude in diagnosis may be arrived at, and the seat of the disease ascertained. On this subject it may be affirmed that, although clinical observation combined with pathology has done much, more requires to be accomplished. As a general rule, it may be stated that disease or injury of one side of the encephalon, above the decussation in the medulla oblongata, es-

pecially influences the opposite side of the body; whilst, if the spinal cord be affected below the decussation, the influence produced is not crossed, but direct. It is said that some very striking exceptions have occurred to this rule, but these at any rate are remarkably rare. Besides, it has always appeared to me probable that, inasmuch as extensive organic disease, if occurring slowly, may exist without producing symptoms, whilst it is certain most important symptoms may be occasioned without organic disease, even these few exceptional cases are really not opposed to the general law. Then, as a general rule, it may be said that diseases of the brain proper are more especially connected with perversion and alteration of the intelligence; whilst disease of the cranial portion of the spinal cord and base of the cranium is more particularly evinced by alterations of sensation and motion. In the vertebral portion of the cord, the intensity of pain and of spasm, or want of conducting power, necessary to sensation and voluntary motion, indicates the amount to which the motor and sensitive columns are affected. Further than this we can scarcely generalize with prudence, although there are some cases, as we shall subsequently see, where careful observation has enabled us to arrive at more positive results.

The fatality of lesions affecting various parts of the nervous centres varies greatly. Thus the hemispheres may be extensively diseased, often without injury to life, or even permanent alteration of function. Convulsions and paralysis are the common results of disease of the ganglia, in the cranial portion of the cord. The same results from lesion of the pons Varolii. But this, if it affect the medulla oblongata, where the eighth pair originates, or injury to this centre itself, is almost always immediately fatal.

(4.) *The rapidity or slowness with which the lesion occurs influences the phenomena or symptoms produced.* It may be said, as a general rule, that a small lesion, for instance a small hemorrhagic extravasation, occurring suddenly, and with force, produces, even in the same situation, more violent effects than a very extensive organic disease which comes on slowly. Here, however, much will depend upon the seat of the lesion. Very extraordinary cases are on record, where large portions of the nervous centres have been much disorganized, without producing anything like such violent symptoms as have been occasioned at other times by a small extravasation in the same place. Here again the nature of the circulation within the cranium offers the only explanation, for the encephalon must undergo a certain amount of pressure, if no time be allowed for it to adapt itself to a foreign body; whereas any lesion coming on slowly enables the amount of blood in the vessels to be diminished according to circumstances, whereby pressure is avoided.

(5.) *The various lesions and injuries of the nervous system produce phenomena similar in kind.* The injuries which may be inflicted on the nervous system, as well as the morbid appearances discovered after death, are various. For instance, there may be an extravasation of blood, exudation of lymph, a softening, a cancerous tumour, or tubercular deposit, and yet they give rise to the same phenomena, and are modified only by the circumstances formerly mentioned, of degree, seat, suddenness, &c. Certain nervous phenomena also are of a paroxysmal character, whilst the lesions supposed to occasion them are stationary or slowly increasing. It follows that the effects cannot be explained by the nature of the lesions, but to something which they all have in common; and this, it appears to me, may consist of—1st, Pressure with or without organic change; 2d, More or less destruction or disorganization of nervous texture. Further, when we consider that the same nervous symptoms arise from irregularities in the circulation from increased as well as diminished action, sometimes when no appreciable change is found, as well as when disorganization has occurred, the theory of local congestions in the nervous centres seems to me the most consistent with known facts. That such local congestions do frequently occur during life, without leaving traces detectible after death, is certain; whilst the occurrence of molecular changes, or other hypothetical conditions which have been supposed to exist, have never yet been shown to take place under any circumstances.—*Monthly Journal of Medical Science*, March, 1851.

T. R. B.

54. *Fatal Effects from Chloroform.*—[At a meeting of the Surgical Society of Ireland (Jan. 25th, 1851), a letter was read from Dr. Geo. ROE, of Cavan, containing the particulars of a case of death from the administration of chloroform; and the writer alludes to two other cases in which death supervened on the operation table, or very soon after removal from it.]

James Jones, twenty-four years of age, who had been a patient in the Cavan Infirmary about two years ago for venereal complaint (for which he had used a very light course of mercury, and of which he was discharged cured), was again admitted on the 26th of February last for a disease of the instep and ankle, supposed to be of a scrofulous nature. He said he had felt a little pain in the foot while in the infirmary for the venereal complaint, but he did not think it of such consequence as to require any attention; latterly, however, the pain and swelling had increased so much, that he was obliged to give over his work, and apply again for admission into the infirmary. It remained for a long time indolent, yet very painful. A great variety of remedies, both local and constitutional, were tried without any advantage. The disease proceeded slowly but steadily on to suppuration, unrestrained by any remedies; afterwards, extensive ulceration, with sloughing of the skin and cellular tissue around the ankle and back part of the leg, as well as of the instep, which gave reason to fear the metatarsal bones and the astragalus were probably the seat of the disease. All this morbid action produced, of course, high degree of hectic fever, with severe diarrhoea, alternating with night sweats, great debility, and emaciation. He had also, for a few weeks, severe bleeding at the nose, with some pulmonary symptoms, as general thoracic pain and shortness and embarrassment in his breathing, but no cough. These hectic symptoms had increased to such a degree in June and July, that his life was despaired of. At this time the stethoscopic signs were very unfavourable. There appeared a general dulness in many parts of the chest, as if the external surface of the lung was congested, or hepatized; but he had *no cough*. By a long course of the usual tonic remedies, bark, wine, chalk, opium, &c., he greatly recovered from all his symptoms, having only a most extensive ulceration of the foot and leg, which he saw was quite incurable, and he repeatedly expressed his wish to have the operation of amputation of the leg performed. This Dr. R. consented to, but with some doubt and hesitation, fearing that it would give but a slight chance of life, from the extreme debility of the patient.

Jones was of a very peculiar and very sensitive temperament, yet very patient and uncomplaining, and endeavoured to conceal feelings and expressions which many with less cause of suffering would give way to. For this reason Dr. R. wished by every means to allay and prevent the agitation and excitement inseparable from the operation.

Jones was cheerful, and appeared to be firm and courageous, but when placed on the table the heart's action was very *quick and weak*, but he did not appear faintish, or more pale than usual. Dr. R. then saw Mr. Nalty, the apothecary, measure *one drachm* of the chloroform, in the small minim glass measure, and pour it upon a little folded lint, which was placed in an oval hollowed sponge, held in the hand with a small towel. Recollecting this chloroform had been used in another case, and finding some little delay in producing the anæsthetic effects, and supposing the strength of the chloroform might be a little weakened, as the bottle had not been kept very closely stopped, Dr. R. directed Mr. Nalty to add *thirty drops* more to that already on the lint.

Dr. R. then applied the sponge, &c., to the patient's nose, directing him to keep his mouth shut. Mr. Brice had scarcely screwed up the tourniquet, which had been placed previously on the thigh, and while Dr. R. was examining the state of the circulation in the tibial arteries, to prevent the least unnecessary loss of blood, and which could not have occupied one minute—certainly he could not have made or taken *fifteen* inspirations—when Dr. Halpin said the anæsthetic effects were produced. This struck Dr. R. as being unusually quick and sudden, and on removing the towel from the face there was a slight convulsive action of the left eyelids, and the lids partially open, and a small quantity of saliva (froth) at the mouth. Dr. R. felt rather uneasy; and on a more minute and instant examination of the heart, the eyes, muscles of the limbs, &c., found the patient *dead*. Every means within reach were resorted to, to try

and restore animation. The strongest ammonia and hartshorn were applied to the nostrils, the fauces, and palate; cold air, cold water, to the surface; and afterwards sealding water was applied over the region of the heart; inflation of the lungs; general friction of the body; change of posture, were all, in turn and rapid succession, tried, but without the least effect. Dr. R. had no means of making or procuring oxygen gas, and unfortunately his portable galvanic apparatus was not then in order or ready for use; so that he had the sad and painful spectacle of his patient *killed*, as if by a stroke of lightning, in less than one minute, before his eyes.

Dr. R. asks, what, in connection with the chloroform, was the cause of this catastrophe? The patient's bowels had been fully and freely opened the day before the intended operation, and also on the morning of that day. He had taken only a small quantity of bread with his tea, at nine o'clock, three hours before.

He was placed in the recumbent posture, his head being very little raised by a pillow, and he appeared as well on that morning, if not better, than he had been for some time; but he was very weak, and hectic symptoms were very evident, and his former diarrhoea had greatly reduced him; yet he was not more exhausted or weak than others on whom Dr. R. had operated, and successfully, without the use of chloroform. The chloroform was taken out of the same bottle which he had used a few weeks previously for an operation on a delicate woman with strangulated hernia, and with whom he used the same quantity (ziss) on the lint. In this case it was four or five minutes before the anæsthetic effect was produced, and she recovered from the effect of it before she was taken off the table.

"I must say," observes Dr. R., "that I have the impression that the success of my capital operations, and particularly those attended with loss of blood, has not been so satisfactory to me as it was previous to the introduction of anæsthetics. But I have, in reflecting on this, attributed it to other causes, partly moral, and partly physical, believing that the change of food, and of outward circumstances of various kinds, has made a very considerable change in the subjects and constitutions of the class of people with whom I have an intercourse and communion.

"But to return, I would wish to know what way I have erred, and what was the cause of my misfortune in the death of my patient? It may be said the case was not a subject fit for chloroform, on account of his weak and hectic state. Now, this appears to me to be a question requiring all the experience, and all the observation and judgment, which can be brought to bear upon it. If we are not to use such an agent in the *weak*, the *timid*, and highly sensitive patient, for what class of patients do we require anæsthetics? I have had several protracted and painful operations before and since the great discovery of anæsthetics, of ether and chloroform, and without the use of these my patients have borne their sufferings without a stir or a groan, and I have often observed to my assistants that no anæsthetic could have produced greater quiet or composure."

"When we hear the very indiscriminate, and I would almost say the unjustifiable use made of chloroform by every description of practitioner, it is scarcely a matter of surprise that much more mischief is not done; yet the medical records and other public journals furnish proofs enough of its great and fatal danger, sufficient to warn the young and inexperienced from using, without due care and knowledge, such a remedy, and one possessing such awful power over human life. I wish I could say that all the medical records of its use and advantages had been given with that fidelity, truth, and honesty, which such a subject requires. It is not by the relation of a number of successful cases, however great, that the laws and rules which should regulate its exhibition, can be laid down or ascertained. We ought to have a faithful account and record of the unsuccessful, dangerous, and fatal cases in which it has been employed; and I fear many who have extolled its use and benefits have most unjustly and unworthily suppressed its dangers, bad effects, and even its fatality in their own hands. I have never used it in midwifery practice, but, from my knowledge of it, I should say that some of our brethren in the north have used it in that branch of the profession to an extent that neither experience, prudence, nor common honesty would warrant."—*Dublin Medical Press*, March 26, 1851.